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ERRATA in the Proceedings of the Royal Society in the last Number of this Journal.
In the abstract of Dr Davy's paper, p. 378—

Line 7 *for* influence *read* inference.
11 ... cork ... coak.
19 ... mixture ... nature.
22 ... cork ... coak.

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THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL.

A Period in the History of our Planet. By LOUIS AGASSIZ, Doctor of Philosophy and Medicine, LL.D. of Edinburgh and Dublin, Knight of the Order of the Red Eagle of Prussia, Professor of Natural History in the Academy of Neuchatel, &c. &c.

THERE is something peculiarly fascinating to the human mind in inquiries into the past history of the human race. What power has placed Man upon the earth, and called him to be its lord and master? Was he the first product of that creative energy which called forth the thousands of living beings upon our planet, or was he preceded by other creations? And what was the conformation of this earth before the energy of his mind, and the labour of his hands, had impressed upon it the indelible stamp of his existence? It were easy to multiply these questions, but difficult to give a just solution of them: for the obscurity that conceals the early and remote yields but with difficulty to the torch of inquiry, and, when we even succeed in penetrating a step or two—in opening a slight path to the succeeding inquirer, the environs of this path become enveloped in darkness still more dense. Where it is man himself that still speaks to his fellow man, where he himself relates to him the history of his fortunes, we may no doubt more readily speak of understanding, of a right comprehension of occurrences; in a word, of knowledge of

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the past. But when the sources of history dry up in the infancy of nations, or only bubble up in the faint glimmer of enchantment and fable, the answer to our curious inquiries becomes more difficult, and the supposed resolution more doubtful. But who can resolve the enigmas which lie buried in the dark night of Time, antecedent to the creation of our species? Who is in possession of the spell that is to raise such hidden treasure?

"Where men are silent," says an ancient proverb, "stones must speak:" and daily experience adds new confirmation to the saying. And they speak to us, the stones and the rocks, the mountains and the valleys; but each has its own language, and each its own modes of speech; and like the tribes of America, none understands the language of the other, nor is it given to every human intellect to acquire the language of all, and in their peculiar expressions, to find the answer to his queries. Is it to be wondered at, that there is still so much interrogatory, and so little answer that is really understood?

But what has been done awakens the hope that something still better and more beautiful will be discovered: let then every one who can contribute his mite to the increase of the general knowledge do so fearlessly: however small it may be, it will not be lost.

It will easily be perceived, that I refer to Geology; and in fact, it is this science, and it alone, which promises to give us, some time or other, a satisfactory answer to the queries which we stated at the outset. It is the only positive science which endeavours laboriously to wring from the past, what it has long shrouded in a veil of nocturnal obscurity; and what it does not find here above, it beats, hammer in hand, out of the obstinate rock, by the dim light of the miner's lamp. It takes the torch from the hand of the historical antiquary, to penetrate still deeper into the obscurity which is unenlightened by mythology or tradition. For it has taken the history of the Earth for its problem; it will enquire what was there when man was not yet there; what lived before creation had crowned its work, by the formation of that being who alone can render himself intelligible to his race, by language and writing, across remote space and time.

Formerly scarce attended to, it is only in very recent times that Geology has risen to the rank of a science. The solution of the enigmas was sought for in a different way; people either rested satisfied with what they regarded as immediate divine revelation, or sought to gain their end by metaphysical investigation, and endless sets of inferences, without being very particular about having a foundation for them in fact. To interrogate the Earth itself about its history, was a notion which was late of occurring, but which, once it did occur, was so much the more zealously acted on; and we may now say, that Geology, like all new sciences, has for a while become the fashion.

And a most comfortable thing to be sure is such a science, in which the greater part still remains to be done, which as yet possesses no history, or at best, the history of a few decades! Moreover, it is a science, for the prosecution of which, collections are necessary. One can gratify one's dilettanteship under the semblance of performing a service to science. Which of its sisters, then, can contend with it in the possession of such captivating qualities? Not one of them! And it bids fair long to remain the bosom-child of scientific amateurs, and of the rich Mæcenases of poor naturalists. May it derive from them all the benefit that it can derive, before they become tired of their plaything, and cast it aside for a new one!

I hope I shall not be accused of seeking to follow the fashion, in introducing a subject belonging to this my favourite science, which I have pursued with great predilection, and which I am now desirous to treat of, because it perhaps, more than any other geological subject, trenches upon every-day life, and is thus interesting for a wider public. The matter in hand is not an epoch, which lying at an immense remoteness, comes scarcely into indirect relation with our present one; it is an epoch of which the vast remains still stifle whole tracts and provinces under their deadly influences, and oppose themselves, as it were, like powerful dykes, to the progress of civilization; an epoch of which the remains attract so many tourists to our native Switzerland, who, lost in admiration of their sublimity, never dream, that what excites their astonishment, is but the ruins of departed greatness.

It is to the *Glaciers*, and the parent of that gigantic family —the *Glacial period*, that I wish to draw the attention of my readers.

Through the inquiries of Geology, the conviction has become universal, that our earth has not always existed in the same form that it does at present; that the inequalities of its dry land, and the extent of its waters, once presented to the eye a totally different appearance from what they do at present; that it was only by degrees, and by means of mighty revolutions, the influence of which elevated entire districts and mountain-chains, and depressed others, that the firm crust of the globe received its present form. These revolutions, to inquire into the results of which, in particular cases, forms one of the great problems of geological inquiry, are the pillars upon which the history of our earth is founded, the march-stones which separate the sections of their periods of development, and determine their different ages,—relatively only, no doubt, for the determination, even approximatively, of the absolute length of any such epoch, is still an unsolved problem. We know, that the chalk is more ancient than the tertiary formations, more recent than the Jura series: this we know, with perfect distinctness; but how long the epoch of the chalk lasted, what period of time elapsed during its deposition, this we do not know, nor have we at the present time any data for determining. May we soon receive such by the industry of our geologists!

A peculiar creation animated the earth during each separate period that elapsed between one revolution and another; each period had its own creation, its particular type of organic existence, and, as in the history of mankind, each great epoch of culture receives a particular stamp, by a particular direction of the mind, peculiar to the epoch, so the totality of living beings which existed upon the earth at a certain geological period, and the remains of which we find entombed in its entrails, impresses upon this period a stamp which cannot be mistaken.

Our present geology rests almost solely upon an acquaintance with the remains of these different creations, and for those who are skilled in it, the sight of a single characteristic fossil is often sufficient to determine to what formation its stratum belongs.

However little then—at least for the older formations, down to the tertiary—it can be contested, that each epoch possessed a Fauna and Flora peculiar to itself, and that with the termination of every such epoch, the collective species which formed its Fauna vanished from the series of living beings, to make way for other species, as little can it be doubted, that the creative energy which called all these organisms into existence, approached but by little and little, and through many gradations, to the creation of those forms which enliven our present earth. Nay, it can even be shown, that in the great plan of creation, by which the distinct periods of the appearance of living beings were regulated, the very commencement exhibits a certain tendency towards the end, betrays the issue towards which it is striving; and, in the series of vertebrate animals, the constantly increasing similarity to man of the creatures that were successively called into existence, makes the final purpose obvious towards which these successions are rising. In respect to the invertebrate animals, the inquiries instituted from this point of view, do not yet suffice to give more than indications of a similar aim in the plan of creation. It is certain, however, that the farther back we penetrate, —the more ancient the formations that we investigate,—so much the more dissimilar do the forms become to those now existing, and so much the less can there be any idea of their identity with creatures of our epoch.

This increasing dissimilarity of forms, in proportion to the age of the strata, is so striking, that a recognition of it has misled many to the false conclusion, that nature at first made but very crude and imperfect attempts in the production of living beings, but that afterwards, upon perceiving her errors and short-comings, she had destroyed the work of her hands, and called a new creation into existence, until after many unsuccessful attempts she had at length succeeded in calling into life the crown of her efforts—man, and a creation corresponding to his wants. This false inference rests upon the same one-sided apprehension of facts, as the view which discerns, in our present creation, a repetition of these attempts, and assumes a scale which the idea of the perfect organism is to ascend, in order to be finally developed in man in its fullest blossom. There can be no doubt, that a polypus, or a worm, is more imperfectly organized than a mammiferous animal, or man,—more imperfect on principle; but does it follow from that, that these creatures are more imperfectly organized for the situation to which they are destined? Is man a more perfect being in the water, than a fish is in the air? Certainly not.

And what is applicable to the gradual development of the idea of the perfect organism in our present creation, taken collectively, can also be shewn to be applicable to the development of the same, through the gradations of the geological epochs.

If we must admit that the organisms of the greywacke, viewed as a whole, are more imperfect than those of the Jura formation, and the latter again more imperfect than those of the tertiary period, so, on the other hand, we must not lose sight of the fact that they correspond most exactly to the circumstances under which they lived, and that they were just as perfect under those circumstances as our present creation is in the present epoch. Would it have been a better adaptation to have placed men upon the narrow islands of the Jurassic ocean, instead of those uncouth reptiles which peopled its shores? Or ought the clumsy pachydermata, which grubbed through the morasses of the tertiary period, to have swam about in the hot seas of the greywacke?

Let us endeavour to give, in a few sketches, a view of the more ancient epochs of the earth's history, in order to arrive, in the simplest manner, at what is to form the immediate subject of our inquiry.

We cannot here attempt any thing like a detailed view of the formations with which geology makes us acquainted; we can only indicate by a few rough strokes the principal points in the grand picture of the earth's history. There can, therefore, be no such thing as any very strict geological sifting. I here unite, as one formation, *the first periods* *of life to the conclusion of the coal period*, in which, no doubt, three periods might be distinguished, during which fish appear as the highest development of animal life. In like manner, I comprehend the collective series of strata, from the *Rothliegendes* to the *Muschelkalk* and *Keuper—the series of the trias formations*—as a *second* grand epoch; the *Jurassic period*, with all its subdivisions, as a *third;* the *chalk* as a *fourth* grand epoch—three periods in which the ugly race of reptiles gains the upper hand, and presents itself at the summit of creation; and, lastly, the *tertiary period*, and the subsequent epoch of what is called the *diluvial formations*, in the course of which the mammiferous animal gradually acquires its present conformation, and paves the way for the creation of man and the race of beings that now encircle him.

Whatever may have been the original form of the earth, every thing indicates that, when the first organic creation enlivened its surface, the latter was overflowed with widely extended but shallow seas, from which emerged only a few islands, scarcely elevated above the surface of the waters. A wonderful animal creation inhabited these oceans; vegetable organisms, no less striking, covered the marshy land. The uncouth Orthoceratites, huge cuttle-fish, trailing after them the shells which serve as their dwellings for a year, with their tiny kindred, the unnumbered heaps of Goniatites; quantities of complexly organized Terebratulæ (which have hitherto been placed in a class by themselves, but improperly, as they may be regarded as merely a peculiar family, though the lowest, of the class of Acephala); grotesquely formed Polypi and Encrinites, attached to the shallow beaches, present themselves as the earliest organisms in the two great types of molluscous and radiated animals. The series of the articulated animals, the dead remains of which, no doubt, had no such solid portions to oppose to corruption as the animals of other classes, finds itself, however, represented by scorpions and the strange Trilobites. How anomalous the form of these fossil crabs was, as compared with the order of Entomostraca, is apparent from the circumstance that naturalists did not know for long what to make of their bodies, until a more exact comparative inquiry demonstrated them to be crustacea similar to the Aselli and Limuli, but whose type, no longer preserved, makes way for other and more highly developed forms.

Strangely formed fishes, armed with hard bony scales against the attacks of their own species, lived and moved amidst the hot waters of the greywacke; and it is remarkable that all the fishes of the greywacke and coal formations belong to the Placoidians (Rays and Sharks), and to the strange looking Ganoidians, whose vertebral column, bent upwards in the caudal fin, presents a configuration impressed upon the full-grown fish which, at the present day, amongst bony fishes, is found only in embryos, and the structure of whose teeth distinctly shows that they lived principally upon hard mollusca, which they crushed with their flat teeth, or on the putrid remains of plants and mollusca. Many of these remarkable fishes, and especially some from the old red sandstone, have, from the uncommon prolongation of the articulated coverings of their gills, been described and delineated as insects, as water-beetles.

In the first creation, then, was formed the germ of the vertebrated animals, that fourth division in the domain of the animal economy, which was afterwards to receive in man its highest development.

The gigantic ferns and monocotyledones, the remains of which at the present day afford, in the shape of coal, the most powerful lever of human civilization, covered the dry land. They displayed little variety of form; but if the species were not numerous, and if their type was limited, the want of variety was made up for by the large quantity of the individuals.

This first creation vanished away; its animal remains were entombed in the stony strata that were deposited at the bottom of the waters; its forests were sunk and covered in the abysses. Separate islands rose above the surface of the waters; previously existing ones became larger; the dry land increased in extent. And beside the Ganoidians of the Mansfeld slate, and the other curious fishes of the variegated sandstone and the muschelkalk, whose jaw-bones some have even ascribed to Mammalia, beside these obscure organisms, there lived huge sea and land reptiles: the Nothosaurus and Dracosaurus, two large sea lizards, allied to the Plesiosaurus, hunted for their prey in the high seas, whilst other salamander-like species, as the Labyrinthodon, watched for their food upon the shore. As yet the other family types of the Amphibia are wanting; it is with the Jura formation that these first gradually come into life. The trilobites have already vanished, and are succeeded by beautiful extinct families of long-tailed crabs; the stalked encrinites, those animal water-lilies of the ancient ocean, are developed in their loveliest bloom. Exquisitaceæ, Coniferæ, and Cycadeæ, together with a few ferns, covered the soil.

The interior of the earth raged anew. As parallel dams the mountain ranges of the Black Forest and the Vosges arose, together with those of the Thuringian and Bohemian forests, to bound the shores of the Jurassic sea, in which a new life commenced. The mighty Ichthyosauri, whose organization, as the name implies, comes so near to that of a fish; the long-necked Plesiosauri, with their short paddles; the Pterodactyles, to which the majority of naturalists still assign an improper place, regarding them as aerial animals, as flying reptiles, whilst their whole organization shews them to have been aquatic animals; these animals, termed by Cuvier the most bizarre of the whole early creation, inhabited the waters of the extensive oceans.

In conformity with their whole constitution, these, in many instances, gigantic devourers had to seek their food amongst the fishes of the sea; and we can still recognise in the coprolites, their petrified excrement, the scales and bones of the fishes and reptiles which served as their nourishment, and, from the mass which fills up their ribs, infer the extent of their stomach and their appetite. In contest with these, innumerable hosts of sharks and other animals, approaching nearer to the present race of fishes, but still belonging to the classes of Ganoidians and Placoidians, ranged through the waves. Numberless masses of naked cuttle-fish, whose back bones form, as belemnites, extensive stony strata, and give evidence of the immense mass which existed of these creatures, meet us here for the first time; they appear anxious to supplant the family of Brachiopoda with chambered cells, which peopled the ancient oceans; in fact, the more anomalous forms, as the Orthoceratites, have already vanished, and only the beautiful spirally-coiled ammonites and nautili retain their place in the ocean, clad in their coat of mail, while all around them are naked. The most various forms of all families of shells and echini peopled the sea and coasts, whilst the most beautiful polypi and stalked echinodermata occupied its cliffs and banks, and various kinds of tortoises, with gigantic lizards, allied to the crocodiles, which often remind us of the present huge pachydermata of the tropics, followed their prey upon the shores.

And what collections of crabs, dragon-flies, beetles, worms, and other forms of articulated animals has not the untiring zeal of inquirers brought to light! Tropical vegetation also had taken root upon the dry land, although not to such a great extent as in the preceding period. In fact, sea algæ and the strange-looking Cycadeæ, with a few coniferæ, are the sole witnesses that the vegetable creation, although overpoised by the animal, was not entirely suppressed.

The domes and ridges of the Jura arose as strong dykes, and formed, along with the previously elevated mountains, the shores of the seas whose larger inhabitants, visible to the naked eye, lie included in the masses of carbonate of lime which, on account of their peculiar constitution, were formerly regarded as the purest examples of continuous deposits from water containing lime. I say formerly, because now, by the help of the indefatigable Ehrenberg, we see in those formless deposits of the chalk most astonishing collections of innumerable microscopic animalculæ, whose calcareous shells, heaped up in measureless quantity, constitute immense ranges of mountains.

But apart from these organisms, invisible to the naked eye, what important progress is made towards the perfection of the types which now people our earth. The radiated animals free themselves more and more from the ground to which they were attached during the earlier epochs; the encrinites in their various forms, which still inhabited the Jurassic seas, give ground, and the varied forms of sea-stars, whose arrival was hardly announced by a few feeble indications in the limestones of the Jura, take for the most part their place. On the sequence of the families of molluscous animals and their geological development, notwithstanding the endless series of catalogues of the petrifactions of the chalk, we possess unfortunately no sufficient data; yet the character of the cephalopods especially is considerably modified, and the other families of shells lose more and more the heterogeneous appearance of the older epochs. The crustacea have no longer, as formerly, the variety of extinct races; in short, there is every where visible a farther step towards the limit of hitherto attained perfection. By dint of my various comparative inquiries into the subject of fossil fish, I have succeeded in estabishing, that in the development of the type of the vertebrated animal, the chalk forms an essential march-stone, and the separation is here more sharply carried out than in any other formation. The ancient fishes were covered either with firm bony scales, overlaid with enamel, similar to the scales of the now existing Sauroids, Polypteri, and Lepidostei, or their skin was similar to that of Sharks and Rays; it is in the chalk that there first appear the representatives of the two great divisions which now form the preponderating majority of species, the fishes with horny, indented or entire scales, the Ctenoidians and Cycloidians. But only a few species are found belonging to genera which now inhabit our seas or rivers; most are so anomalously formed, that I was obliged to frame new genera and families for them.

But it is not in the class of fishes alone that this progress of vertebrated animal development is manifested. Another very important circumstance has recently been discovered, which assigns to the epoch of the chalk an important part in the development of creation.

By a minute examination of the fishes contained in the celebrated Glaris slate, which was formerly considered a very old formation, I have been able to demonstrate that it belongs to the chalk; and that it is only through alterations caused by violent plutonic influences, that it has acquired its present peculiar appearance. And these slates contain remains of birds! The feathered denizens of the air have left their tender legs in these slates as evidence of their existence. It is to my friend Escher von der Linth, the indefatigable student of the Alpine formations, that we are indebted for this invaluable discovery, equally important to palæontology and zoology; and not the slightest doubt can exist that the discovered fossil is actually a bird. Only the last step is now wanting in order to mount to the highest degree of vertebrated animals, and in the epoch succeeding to the chalk this is done; we meet with the mammiferous animal in the strata of the tertiary formations.*

Until Cuvier, the greatest naturalist of our times, and Alexander Brongniart, directed their immortal labours into the field of this formation, it was not properly distinguished from the more recent alluvia. It was they who first, by an accurate comparison of its fossils, and especially those of the Paris basin, made us circumstantially acquainted with the Fauna of this formation. Their work still remains a model; and Cuvier's inquiries into fossil bones form the most exalted monument of human acuteness in science.

Some essential points bring the tertiary strata into the closest connection with the circumstances to which the present crust of the earth is subjected. Whilst the older formations, upon whatever part of the earth they are met with, present a surprising similarity in their Faunas, so that, apart from the differences which the configuration of their outline must bring along with it, the same remains are found in similar strata, in either hemisphere indifferently; and whilst, consequently, this equality in the distribution of living creatures over the earth indicates for the earliest epochs a more equal extension of temperature, at the poles as at the equator, we find, on the other hand, in the tertiary period, and perhaps even earlier, distinct signs of the formation of different climates, impressing upon different zones a different organic character. Yet this peculiarity of climates is by no means so distinctly impressed upon the proper tertiary formations, as in the diluvium or our present epoch. The stratum still retains the same general zoological character in all places where it is found, although considerable modifications are perceived in the details.

A second great step is the exact distinction which can be drawn in the tertiary period betwixt formations of the sea, of fresh water, and of brackish water. Let it not be objected, that attempts have been made to separate salt and fresh water formations in earlier strata. This has no doubt been done, but without much foundation; and the animals which ought almost exclusively to determine such a separation—the fishes—do not indicate such a difference until the tertiary period. Nay, my inquiries into the fossil fishes would rather lead me to conclude that the ancient seas were fresh water oceans; and that it was at a comparatively later period, and probably through plutonic agency, that sea water acquired its quality of saltness. Perhaps this agency may have exerted no inconsiderable influence upon the series of development of the different Faunas.

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* The small marsupial mammalia which Buckland discovered in the Jura formation of Stonesfield, still remain an isolated phenomenon.

The genera of molluscous and articulated animals, which at the present day have representatives amongst living species, become more and more numerous. The ammonites, those voracious brachiopods, whose beautiful shells we meet with in all strata until the period of which I am now speaking, vanish as it were all at once; and their place in relation to the other lower animals seems to be supplied by the carnivorous piercers, whose number is uncommonly increased, and of whose destructive appetites so many tertiary shells afford at the present day the most decisive proof in the holes bored through their masses.

But what particularly characterises the tertiary beds is the various remains of the bones of gigantic mammalia which are found in th[illegible], and which Cuvier's creative hand made to arise a second time out of the dust.

These bones, the accidental finding of which gave occasion to all the strange tales about giants of earlier ages, nay, as Unger has recently proved, to many of the fables about dragons and other monsters, shew in the most evident manner how, even in the mammalia, nature gradually elevated herself from the ungainly, and, as it were, the grotesque, to the actually beautiful, the symmetrical. The continent, to be sure, had not yet assumed its present form; arms of the sea stretched far up into the land where there is now solid ground; the chain of the Alps had not yet arisen; but still, those internal formations predominate which were deposited in large fresh-water basins and extensive marshes. Only two of the many orders of mammalia were represented by anomalous species—the Cetacea, the giants of the sea, by species similar to the existing *Manati*; and the Pachydermata, inhabitants of the marshes and plains, by the strange forms of the *Palæotherium* and *Anoplotherium*, which stand half-way between the horse and the tapir, and the forms of which, as restored by Cuvier, have passed into all penny magazines and family journals, and are known to every body.

However, the Molasse and Diluvial periods were richer in forms than that of the *Calcaire grossier*. Apes of considerable size peopled the forests of Europe as at present those of the warmer zones; nimble deer and elks scoured through the thickets of the plains; huge rhinoceroses and hippopotami wallowed in the marshy lakes or on the banks of rivers; the uncultivated meadows were trodden down by herds of wild elephants; babiroussas and swine of different species grubbed through the mire of the waters; the huge Dinotherium, with its downward-turned tusks, scraped up the banks of the rivers to provide itself the masses of vegetable nourishment which its gigantic body daily required; timid hares squatted in their forms to avoid the foxes and martins, which occupied the same ground that they did. But, if the peaceful race of the Graminivora was developed in the gigantic forms of pachydermata and ruminants, their enemies were neither less numerous, nor less on a par with them in strength and force. The caverns of the mountains are filled with bones which their inhabitants dragged in and gnawed there; ravenous hyænas, clumsy bears, lions, tigers, dogs, and cats of every species, were the rapacious exterminators of the elephants and rhinoceroses, as of the defenceless deer and antelopes. And it was not only in the warmer situations which their congeners now inhabit; no, under our own latitude, on to the steppes of Siberia, did all these creatures of a milder climate exist. And where, formerly, elephants found their nutriment, and hippopotami sufficient pasturage, there are now extensive plains which scarcely support the nimble rein-deer, where the surface thaws but a few weeks in the year, and scarcely affords existence to the Iceland moss! The earth was warmer where it has now become cold, and its heat was more equally distributed. Yet, on a comparison of the Faunas of different zones, we perceive the most distinct boundaries of climates which, in a remarkable manner, stand in a certain relation to the climates of our epoch. For, when elephants, hippopotami, and rhinoceroses inhabited the Old World, the savannas of South America were peopled by the gigantic Edentata, those inhabitants of caverns whose awkward persons were for the most part protected by a hard coat of mail against the attacks of animals of prey, to which they must else have become, from their unwieldiness, an easy prey. The Megatherium, of which the skeleton attracted so much attention, that even the Spanish government was at the expense of conveying it to Europe, I shall adduce as the sole type of the strange Brazilian Fauna, with which we have very recently been made acquainted through the indefatigable inquiries of Doctor Lund.

And New Holland,—the land of wonders, with its bizarre forms of men, mammalia, and plants, the native country of the Ornithorynchus, of the Kangaroo, of the Echidna, of the most various Marsupialia,—preserves this seal of the wonderful, even in this earlier epoch; for the zeal of inquiry has already discovered bones of gigantic fossil kangaroos; and progressing civilization, whilst it seeks to turn to account the productions of the soil, will likewise send us from thence valuable contributions to science.

Such creatures were existing, when an unexpected catastrophe put a period to their existence. A climate, such as the poles of our earth can scarcely produce,—a cold in which every thing that had life was benumbed,—suddenly appeared. Could the animals which were created for a moderate tropical climate survive such a thorough change? Certainly not; for nowhere did the earth offer them protection against the omnipotence of the cold. Whithersoever they fled, into the dens of the mountains, which formerly had served to many of them as a lurking-place; into the thickets of the forests; everywhere they succumbed to the might of the annihilating element. The aqueous vapours which the warm atmosphere of the earth must then have contained in greater quantity, and the quantity of which was undoubtedly in proportion to the greater extension of the waters, and especially of the large internal lakes and morasses of the diluvial period, were, upon that sudden change of temperature, deposited in a solid form. A crust of ice soon covered the superficies of the earth, and enveloped in its rigid mantle the remains of organisms, which, but a moment before, had been enjoying existence upon its surface. In a word, a period appeared in which the greater portion of the earth was covered with a huge mass of frozen water; a period in which all life was annihilated, every thing organic upon the earth was put an end to,—the Glacial period. It is this period of our earth, to enquire into the existence of which, and its intrenchment upon our present epoch, I have long assigned myself as a problem; whose existence the men of science at first would not even give themselves the trouble to deny, till the force of truth obtained a triumph over many, if not over all, and constrained a recognition of the justness of what used to produce only a compassionate smile as the lamentable aberration of an overstrained fancy.

This glacial period is the epoch of separation betwixt the Diluvial period, as it has been termed by geologists, and our present period; it is it which, like a sharp sword, has separated the totality of now living organisms from their predecessors, which lie interred in the sand of our plains, or below the ice of our polar regions; lastly, it is it which has left to our times the testimonies of its former greatness upon the tops and in the valleys of our Alps,—the glaciers.

If, upon a fine calm morning of spring or autumn, we ascend the southern declivity of the Jura, it frequently happens that a thick mist still covers the plains of the valleys; whilst above, the enlivening sun already sends down his uninterrupted rays, and the sky is resplendent with the loveliest azure-blue. Nothing can the enquiring eye discover in the depths below. A white cloudy mass, often glancing like silver in the sunbeams, covers the large plain of Switzerland, with its smiling green, its cheerful meadows, its blooming villages and cities. Scarcely does a gentle undulation of the surface betray to the attentive observer how lightly mobile is the veil whose thick web conceals the covered beauties from his eye. Opposite to him glance in the remote distance the colossal Alps attired in their snowy garments. Clear and distinct is their outline relieved against the deep azure of the cheerful sky, and high stretches their summit, whilst their base is encircled with the impenetrable mist-wall which hides the doings of man below. So peculiar is the impression of this picture, bringing us so completely in contact with nature in her simple grandeur; so strangely is the susceptible mind struck with this vision, where nothing betrays the life which everywhere else plants its traces on our environs, that whoever enjoys this sight for the first time must preserve it amongst his most pleasant recollections. Such, I conceive, if I may be allowed to compare so small a tract with what was boundless, such may have been the appearance of our earth, when, at the glacial period, a rigid snow-crust covered its surface.

The earth had already assumed its present contour, with the exception, perhaps, of the principal range of the Alpine chain, and of the mountains that rose simultaneously with it: Mont Blanc had already raised its head above the surrounding plains; the broad ridge of the Jura, the Vosges, the Black Forest, the mountains of England and of Sweden, had been the witnesses of its elevation. Then numbness seized the light "sailors of the atmosphere," the clouds and vapours; icy winds drove them in a solid form to the earth, and, like a huge winding-sheet, they enveloped the polar regions, the north of Europe and of Asia. The British islands, Sweden, Norway and Russia, Germany and France, the mountainous regions of the Tyrol and of Switzerland, down to the happy fields of Italy, together with the continent of northern Asia, formed undoubtedly but *one* icefield, whose southern limits investigation has not yet determined. And as on the eastern hemisphere, so also on the western, over the wide continent of North America, there extended a similar plain of ice, the boundaries of which are in like manner still unascertained. The polar ice which at the present day covers the miserable regions of Spitzbergen, Greenland, and Siberia, extended far into the temperate zones of both hemispheres, leaving probably but a broader or narrower belt around the equator, upon which there were constantly developed aqueous vapours, which again condensed at the poles; nay, if Tschudi's observations in the

Cordilleras and Newbold's at Seringapatam shall be confirmed, and to these we may subjoin those made by earlier travellers upon Atlas and Lebanon, the whole surface of the earth was, according to all probability, for a time one single uninterrupted surface of ice, from which projected only the highest mountain-ridges covered with eternal snow. The limits which would seem to be indicated by the various phenomena which we shall afterwards treat of, are very probably referable to a subsequent epoch, when the universal ice-crust had already begun here and there to disappear, and particular tracts to emerge like oases in the immense icy desert. Be that, however, as it may, this much is certain, that, upon the northern half of the European-Asiatic continent, there have been found only few, and proportionally unextensive districts, which do not bear impressed on them the traces of former envelopement in ice, and that in the excepted places the configuration of the surface has been decidedly unfavourable to the preservation of such traces.

But it is only a few years since the great importance of the phenomena, which have remained as traces of the glacial period, for the general physical history of our earth, was surmised; the number of naturalists who occupied themselves in searching out these traces was but few; still fewer, I may venture to say, understood how to find them; many would not even see what they did find; and thus it need be nowise surprising that more exact accounts cannot be given respecting the ubiquitous extension of the ice-covering, its spreading towards the south, &c.

We have certain data with respect to the height to which this ice-covering had swollen, and with respect to the thickness which it must have reached in many places. I have followed its marks along the coasts of England, Scotland, and Ireland, and no doubt can now be raised in regard to the fact that in our latitudes the ice extended to below the level of the present sea. At many points of these coasts I have, as far as my eye could penetrate the water, seen these traces deep below the surface;* and so indelible are these traces, so deeply imprinted are their characteristic marks, that the roaring breakers have not even yet been able to erase them.

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* Hence there can be no doubt, that, if a north sea did exist at that time, its level must have been much lower than it is at present; for it is established, by

On the other hand, the ice has imprinted upon all the mountain tops of Great Britain, which in Ben Nevis rise more than 4000 feet above the level of the sea, the stamp which attests its former presence; and there can be no doubt that its colossal masses were piled up above the highest summits of these mountains.

In like manner, on the high ridges and peaks of the Alps, the thickness of the mass of ice can be directly measured; at least we are able to determine to what absolute height the solid glacier ice extended. How high the *Firn*, and the snow which perhaps rested upon it, may have reached, we can now have no exact knowledge, as it is only the solid ice that scratches the traces of its presence upon the rocks, whilst the light snow and the granular incoherent *Firn* leave no mark of their existence. By various barometrical measurements I have found that in our Alps the limits of the solid ice on the sides of valleys, the bottom of which is from 2000 to 3000 feet in absolute height, and which are now entirely free from glaciers, reach to a height of more than 8000 feet above the level of the sea.

What a huge mass, transcending our utmost conceptions! Admitting, what in fact must be assumed, that the covering followed in its extent the different inequalities of the surface, that, consequently, the circumstances of traces of ice being found in the Alps to such a height affords no proof of an every where similar absolute height of the icy surface; that it affords

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recent scientific accounts respecting the glaciers and ice-fields of the polar regions, that the masses of ice which are so formidable in the high northern latitudes as floating ice-bergs, are fragments of glaciers, which, stretching out from the solid land into the sea, are undermined by the waves and deprived of their basis, until at length gravity overpowers their coherence with the masses which rest upon the land, and the block falls into the sea. It results from a consideration of this phenomenon, as well as from direct observation, that, as far as the sea reaches, the glacier ice is dissolved, and, consequently, it does not come in contact with the bottom of the sea.

The north sea of the glacial period had therefore either a much lower level than at present, or else, as seems to be almost proved by a consideration of the whole phenomena, its basin was filled to the bottom with solid ice.

no proof that an icy burden of 8000 feet in thickness rested upon the plain of Switzerland, the provinces of Germany, &c.; retrenching and deducting as much as we can find possible, in order not to terrify our imagination with the grandeur of the object, there will still remain something immense, to which our knowledge of the present world would in vain seek to present any thing parallel or similar.

It would, however, in my opinion, be an erroneous conception of the consistence of this ice-crust, to suppose it to be composed of a solid mass. Where there prevailed such an uncommonly low temperature as was necessary to envelope the earth to such an extent in a frozen covering, all the necessary elements were wanting to change the loose deposits of the atmosphere into compact ice. For it is only by the addition of fluid water, by the repeated alternations of the thawing and freezing of the snow-masses saturated with water, that these become gradually transmuted into solid ice, which, however, still bears the traces of its origin.

In our high Alps, it is only to a certain height that we meet with compact ice; above that height we find only loose snow, or what the natives call *Firn*, the loose incoherent mass of which leaves no trace of its existence upon the rocks. This is simply owing to the circumstance, that above these boundaries—*the Firn-line*—the temperature never attains for any length of time to such a height as is requisite for the production of fluid water, and so for the formation of solid ice. But in an epoch of general refrigeration like that of the glacial period, the earth collectively came into a state of temperature which must have been analogous to that of the Alps at the present day, that is, the Firn-line descended in proportion to the refrigeration, and probably so low, that, in the temperate zones at least, even at the level of the sea there existed only snow or Firn, but no ice, the temperature, standing almost constantly below 32° Fahr., not being sufficient to call forth a formation of solid ice by the melting of the upper layers of snow.

But if such a partial and superficial thawing of the measureless snow-fields of the glacial period, and their transition into compact ice was impossible, it follows that there could be no moving of ice-fields in any direction during that period; and I must lay great stress upon this point, because it presents an essential difference betwixt my views and those of various other men of science, who likewise seek to explain the transport of erratic blocks by masses of ice, but in an anomalous manner. During the glacial period there was no motion; not a brook, not a rill furrowed the surface of the snowy covering, to remind by its purling that all life had not yet become torpid. Scarce could the sun, formerly so quickening, soften the surface of the snow by his most powerful rays; water, so far as the ice-covering extended, existed only in a frozen state. But according to our present state of knowledge, there can be no such thing as motion of the glacier ice, until the warmth of the surrounding media has become such as to cause the melting of the superficial strata. Just as it is only under its exciting influences that life in general can exist, and as without its beneficent operation life must cease, so the only life which the glacier exhibits, the single phenomenon by which it seems to take part in the general life of nature—*motion*, demands warmth as the medium and condition of its existence. It was not, therefore, until the time when a return of warmth, from whatever source it may have come, smiled upon the torpid globe—when the sun began to exercise its powerful influence with renovated force—when under its warmer beams the crust began to dissolve, the ice to soften—it was not until then that all these grand phenomena could take place, of which the surface of our present earth is the witness. What, therefore, the majority of geologists have hitherto believed to be the result of immense floods and currents, and a few to have been caused by the increase and the progressive motion of glaciers, namely, the transportation of erratic blocks, of alluvial boulders, and the polishing and channelling of rocks—these are to me manifestations of the retreat of the glacial period, phenomena which denote the moment when an alteration in the climate of our earth began to confine the cold within those narrow boundaries which it now occupies—phenomena which denote the places whence torpid winter began her slow retreat towards the rocks of our lofty mountains, and to the ice-bound continents of the north.

Manifestations of retreat? will it be asked me. *Whence*, then, the retreat? For that there *has been* a retreat, cannot well be a matter of doubt to us, whose life could only exist by dint of, and subsequent to, such a retreat. The fact is just as undeniable as its causes are hypothetical. A short time ago I could have assigned a probable cause, founded upon the beautiful investigations of one of the ablest of our geologists, Elie de Beaumont, and on his confident dictum, that the Alps had arisen from the bosom of the earth at two different epochs; Mont Blanc, with its chains to north and southward, being more ancient than the chain of the Eastern Alps, which were the most recent of all the elevated mountain chains, and formed a wall of separation betwixt the diluvium and our present epoch. Just as the violent revolutions, whose results were the elevations of more ancient mountain chains, had on every occasion swept off a phasis of developement of animal life upon the earth, in order to make room for one newly commencing, so, I imagined, might the elevation of the eastern great Alpine chain have been that last struggle of the powerful internal forces of our globe, which had put an end to the glacial period, and rendered possible the existence of our present creation. Following E. de Beaumont's views, I have in my last published work* expressed the following opinions:—"That only Mont Blanc, with the chain of the maritime Alps, existed when the ice enveloped the northern hemisphere; but that when the chain of the great Alps rose from the bosom of the earth, the icy covering which had rested upon the position they now came to occupy, was elevated along with them, formed thus an inclined surface, and was covered with the ruins of the rocks which were shattered by this immense revolution; that the temperature of the earth was altered by this elevation; and that a consequence of this alteration was the retreat of the ice covering to the northern regions and the Alpine mountain summits."

More recent inquiries, however, of two friends, which, to be sure, have not yet been made public, but the result of which has been communicated to me privately, afford strong grounds for doubting the fact of the elevation of the diluvial strata by the Eastern Alps, which De Beaumont makes the pillar of his theory, and lead rather to the supposition that the Eastern Alpine chain arose contemporaneously with that of Mont Blanc, and anteriorly to the formation of the diluvium, and consequently, that it already existed in its entire grandeur when the glacial period commenced. If the result of these last inquiries, so opposite to that of M. de Beaumont's, shall turn out to be correct, we can assign no cause proceeding from the interior of the earth, no geological cause, if I may use the expression, for the retreat of the ice coverings, but must seek it, where we must seek likewise for the causes of the appearance of a glacial period, in the empire of conjecture, or in the mean-time rest satisfied with the facts, without knowing their connection with the history of the earth in general.

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* *Untersuchungen über die Gletscher.* 1841.

Whether, however, we seek for its cause in the changeable condition of the sun, in a periodicity of the light and heat which it causes, in a change of the atmosphere, in an elevation of the internal temperature of the earth, in a changed position of its axis to the sun, or in an universal definite movement of our solar system in space—and both for and against each and all of these suppositions there are probabilities enough—the retreat *did commence.* But it was not a hasty flight into a lurking corner in the mountains which opened the wide land to creation; it was a slow deliberate retreat which ceded the so long occupied field only inch by inch.

It was the plains which first freed themselves from their torpid covering. Where the long level tracts of Northern Germany, of Russia, and of France extend, there, under the influence of a warmer sun—a sun such as shines upon us at the present day—the snowy covering first began to dissolve, and by the trickling in of the water, and its re-congelation in the interstices of the looser snow strata, to change into more compact ice. With this transmutation of snow into ice, were attended two important circumstances, namely, the movement of the masses of ice in the direction of the superficial slope, and their effects upon the solid ground on which they rested. A stirring life, if I may so express myself, came in place of the torpid inactivity of the frozen masses. Clefts opened under the expansive force of the solar heat, and thus furnished the waters which the melting of the superficial strata called forth in copious abundance with a welcome outlet below, where, in the rolled materials of the soil, they dug themselves beds, bounded and protected by the crystal walls which they had just broken through.

Although at present, in the attenuated atmosphere of our lofty mountains, the evaporation of the frozen masses of ice and snow, especially in warm days, far exceeds what melts into fluid water, the proportion must have been very different on those extensive plains which, less elevated above the level of the sea, stood under a much more considerable pressure of the atmosphere. Moreover, the very slight inclination, or rather the almost perfect levelness of these plains, was not very favourable to the formation of frequent clefts and fissures, so that the glacier brooks composed of the different rills on the surface of the ice, must have been much more considerable than those of our present glaciers, where for the most part a comparatively limited surface, frequent fissures, and smaller pressure of the atmosphere, prevent the accumulation of larger brooks upon the surface of the glaciers. There are but few brooks to be found upon the surface of our Alpine glaciers, over which the practised leaper would hesitate to spring. But at the period I am now discussing, the crystal floods of large rivers dug their mutable beds out of the extensive surface, and when at length, after a long course, a fissure afforded them the long-sought outlet, they threw themselves in magnificent falls into the azure depths, the immense strata of rolled matter and of sand which the icy mass by its grinding movement collected below it, grubbing through to the bottom. Circumscribed by the ice-walls, these glacier rivers gnawed vaults for themselves under the icy covering through which they pressed towards the greater depths which the uneven surface presented, undermined deep beds in the rolled matters, and thus produced valleys of denudation, the direction of which is frequently inconceivable with reference to the present configuration of the surface, and becomes explicable only when we consider that it was ice-banks that determined the direction of these subglacierian rivers, and formed the limits of their beds.*

Thus began the retreat. The centres towards which its course was directed were, on the one hand, the far north; on the other hand, the lofty mountain-ranges of central Europe, which still lie partially buried in eternal snow; and from these regions, from the mountains of Norway and of Sweden, and the chains of the Alps of our continent, descend the rude witnesses of the extension of the ice covering, those huge blocks which lie scattered in the plains of Northern Germany and Russia, of Switzerland, and even on the ridges of lower mountains, as the Jura, and whose origin cannot be doubted, so characteristic is, for the most part, the species of rock which enters into their composition. Torn from the ridges of the Scandinavian chain, these erratic blocks were moved forward on the surface of the ice-coverings over the Baltic Sea, whose basin was filled with ice instead of salt water, and deposited on the plains of northern Germany on the skirts of the ice-field. From the summits of the Alps in like manner, radiating in all directions, moved the fragments which the destroying influences of the atmosphere, perhaps also the elevation of a part of the chain, had broken loose from their bed, and hurled upon the surface of the ice. The plains of Switzerland and the southern declivities, nay, even the inland valleys of the Jura, the vales of Lombardy, and the regions of eastern France, received from the Swiss Alps those blocks which progressing culture has already applied in such quantities to industrial purposes, but of which the quantity is still so great, that centuries will not suffice to blot them from the empire of the existing.

When the retreat of the ice-crust towards the north and towards the Alps began, these blocks were moved thence towards their present situations.

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* I cherish the conviction that the various traditions of ancient nations about immense floods and inundations are referable to those times in the infancy of the human race, when men inhabited only the moderate climates of the tropics, whilst the northern latitudes were still covered with the glaciers of the glacial period. According to this view, these traditionary floods were events similar to the inundations which even now are so often occasioned by the glaciers; only with this difference, that in proportion to the greater extension of those glaciers, they were upon a much larger scale.

I shall here be accused of a contradiction. "How is it possible," it will be said, "that the ice-covering retired just towards the points from which the blocks were moved? How can it have happened that these were conveyed from the centre of the chains towards the periphery of the ice-fields, whilst their bearers, the masses of ice, had a retrograde motion from the periphery to the centres—that is, in a direction diametrically opposite to that of the blocks?"

There lies, no doubt, an apparent contradiction in the truths just announced, but it is only apparent; for the retreat of a mass of ice is nothing but the result of a disproportion betwixt the forces which tend to its destruction on the one hand, and its forming elements on the other, in which the former attain the upper hand. Snow and water are the forming elements, upon the co-operation of which the production and progressive motion of the glacier masses depend; by the constant addition of the former the totality of the latter is increased, and this increase, united to the expansion which the freezing water undergoes, are the causes of the progressive motion, which is properly only an expansion of the mass in a single direction. But if the melting and evaporation of the masses of ice which are occasioned by the heat outweigh the expansion, if more is withdrawn from the mass in a fluid and vapoury form than it gains in the way of condensation of the atmospheric deposits, it must obviously decrease in bulk, and its decrease will be most where these destructive influences operate most strongly; that is to say, at a distance from the colder centres, at the extreme periphery of the masses, where these come in contact with a warmer air and soil. It is accordingly this preponderance of the forming influences over the destructive in the glacier masses, which, to the eye of the superficial observer, appears as the progressive motion; and in the same way it is the depression of the forming elements, which appears as the retreat of the masses; whilst, on a more exact observation of the phenomena, it is found that the mass constantly moves forward, even when a greater decrease at particular places appears there necessarily to imply a retreat.

The same relations must also have come into existence when the ice-crust was exposed to the destructive influences which a condition of heat like ours at the present day must have brought along with it. In the plains began, as we have already remarked, the contest, in which death itself was to succumb, and the new life of our present creation to conquer. Attacked from without by the beams of the sun, and from within by the rays of the earth's own warmth, the ice dwindled away, and the more land became free so much the more powerful became the forces which supported the contest. But just where the ice disappeared, on the boundaries of the ice-field, was the seat of the more active movement, which, finding its point of support in the before-mentioned centres, proceeded from thence in every direction; for the most considerable movement of the ice-masses is always at those places where heat can exert the greatest influence upon them. But if the destructive influences preponderated at the limits, the forming elements did so at the centres, and the masses collected there consequently pressed downwards to the contest as a fresh reserve, carrying along with them fragments of their formation-sites—the erratic blocks. From the distribution of the various alpine rocks upon the chains of the Jura, as well as from the extension of the zones of blocks that descend from the heights of Scandinavia, it will some time or other be possible to calculate the time which was required for the retreat of the ice-covering into its narrower limits.

But vain was the contest. More and more was the ice-mass compelled to recoil, more and more was the land denuded, to become covered with verdure. The plains of Germany, Russia, France, and Italy, were rescued from their frozen state; the Baltic was set free; the north sea, for the most part, was anew covered with waves. But the more considerable chains of mountains, presenting, by their elevation above the sea-level, a more secure resting-place to the ice-covering, still retained the eternal winter upon their summits and in their valleys, and thus there arose *detached* glacier ranges, which no longer formed a connected whole, like their common ancestress, but separate groups, each of which belonged to a particular mountain range. The Scandinavian peninsula, the heights of the British islands, the Alps, the Vosges, and probably most chains similar to these last in height and situation, presented those groups of glaciers, the traces of which have not yet been sufficiently followed to afford a perfect chart, if I may so express myself, of the glacier groups of the retreat. The whole plain of Switzerland was still filled with ice, for its space is too circumscribed, the distance betwixt the two ranges which encircle it, the Alps on one side, and the Jura on the other, being too small in comparison with the height of these mountain-chains, to admit the supposition of a simultaneous thawing of these plains with those of other countries lying lower and of greater extent. The elevation of the Alps is indeed so considerable, that the masses of ice which proceeded from them overflowed, even at the outset, the independent glaciers of the Jura, and that the collective glacier groups which covered Switzerland took a direction from south-east to north-west, that is, an opposite one to that of the glacial movement proceeding from Sweden and Norway. The most evident proofs of this superior grandeur of the alpine ice-masses are afforded by the blocks formed of granitic and other plutonic rocks, which have been deposited principally on the southern declivity of the Jura, and even likewise in its inner valleys, and which bear obvious testimony to the fact, that the motion proceeding from the Alps was continued to the inner chains of the Jura. The level of the Swiss glacier groups sank only by degrees, and from the distribution of the different rocks, the origin of which, in the alpine chain, can be determined with the greatest exactness, especially from that of the very strikingly peculiar conglomerate of Valorsine, tolerably certain data may be collected with respect to the height of the different levels of the ice-masses. Step by step, however, the ice faded away likewise from the plains of our native land before the quickening influence of heat; the blue waters of its lakes rolled their waves unfettered; the molasse hills were denuded of their covering, only the highest peaks of the Jura, as the Dent de Vaulion, and several others, still retained some independent glaciers. But the beautiful alpine valleys, whose wild scenery attracts so many admirers of the sublime, were still filled by the glaciers, which were always becoming more and more subject to the influences which rule them at present. The great valleys of the Rhone, of the Aar, of the Reuss, of the Rhine, of the Adda, &c., which roll their waves from hence towards every possible direction, were covered with immense glaciers, and may have presented nearly the same appearance that they now do in very snowy winters, when their depths are filled up, and only a slight depression betrays to the wanderer the clefts over which he steps with a careless foot.

On the Migratory Tribes of Natives in Central India. Communicated by EDWARD BALFOUR, Esq., Assistant Surgeon. Communicated by the Author.

The hills and forests in the centre of India, are inhabited by people differing widely from the inhabitants of the plains. Their great abode, says Mr Elphinstone, is the Vindya mountains, which run east and west from the Ganges to Guzerat, and the broad tract of forest which extends north and south from the neighbourhood of Allahabad, to the latitude of Masulipatam, and with interruptions almost to Cape Comorin. These people have separate names—Paharias, Kols, Gonds, Bheels, Colis, and Colaris; but in many points they differ from each other, and little has been done to shew that they are the same people. In addition to these races, there are many smaller communities spread throughout India, each with a distinct name, and speaking a distinct tongue; leading a migratory life, and resorting only to towns to purchase a few necessaries, they seem the remains of some aboriginal people who had occupied the soil perhaps before any of the nations now possessing it; and it may not be uninteresting to mention some of the habits of these nomade races.

THE GOHUR, CALLED BY EUROPEANS AND NATIVES BINJARI OR LUMBARI.

The Binjarries are separated among themselves into three tribes—Chouhone, Rhatore, and Powar. Their original country, they say, was Rajputanah,* but they now are spread over Hindostan, all adhering to the same customs, and speaking the same language. This bears a strong resemblance to the language of Guzerat, though there are many words in it without affinity with any of the dialects we are acquainted with. At the head of the Binjarries in the Dekhan are two individuals who receive the title of Naeks. They reside in Hyderabad, and the encampments located near that city refer any disputes that arise to them for their decision; but the chief occupation of these Naeks is to keep up a correspondence with the different parts of the country, to gain early information from localities where war or famine has raised the price of grain.

The Binjarries are grain merchants; indeed the name is given them from their occupation; and their traffic being carried on by bullocks, they traverse the most impracticable countries to collect supplies, which they pour into the districts where scarcity prevails, or they move in the track of large armies, to furnish them with grain during the campaign. In carrying on war in India, where armies carry their magazines along with them, the services of the Binjarries are almost indispensable, and their occupation renders them sacred. For this reason, though moving among hostile bodies in time of war, they consider themselves secure from being molested by any party, and there have been instances of large bodies passing near camps, and though refusing to dispose of the grain they carried, being allowed to move on to the enemy, the dread of alarming them, and thus banishing them for ever, being sufficient to protect them from interruption. The time of hostilities or of dearth was a period of activity among them; but our successes have restored order to India, and have sent our troops to cantonments, and with the return of peace, nothing occurs to interrupt the labours of the husbandman, and scarcity seldom prevails. These changes have done much to make the Binjarries poor, and where disease has swept away their bullocks, the community, unable to purchase others, has broken up and dispersed. When thus reduced, the women bring firewood to the towns to sell which their husbands cut in the jungles. They were at all times considered a bold and formidable race, and while traversing the country with herds of bullocks, transporting grain and salt, they frequently perpetrated robberies in gangs, and they are not over scrupulous in committing murder on these occasions, if they meet with opposition, or deem it necessary for their security. With the approaches of poverty, too, vice has grown apace; many are convicted of stealing cattle and children, and Thugs have also been detected among them.

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* On the summits of the hills (formerly islets) which, united, form the island of Bombay, reside about 75 families of cultivators, who say they emigrated from Rajputanah. Many of the words in the language of this people, and the dress of their women, are similiar to the Gohurs. They call themselves Purmans.

A community of Binjarries is termed a Tanda. In each Tanda an individual is selected to whom the title of Naek is given, but his rank would seem to clothe him with but little authority. No rules exist among them to regulate their conduct or guide their society, and though they keep together in large bodies, it would seem more from their intermarriages and the security numbers give, than from any laws binding them to the tribe. The Tandas in their movements encamp on wastes and uncultivated spots, sometimes near but more frequently remote from towns.

The Binjarries pull down the wild boar with dogs of a powerful and peculiar breed, which they keep in all their Tandas; but with the exception of the wild hog, they live, as regards food, like other Hindus. A few are met with who can read and write. Their wandering life precludes them from residing in towns; they live under tents while the hot weather continues, and on the approach of the monsoon construct grass huts to shelter them from the piercing rains that fall.

Their features are dark and bronzed. The men have tall and muscular frames. Their dress differing much from the nations and communities around them, attracts attention to the females of the tribe, on whom nature has bestowed the most faultless forms; tall and exquisitely moulded, these dark children of the desert move with a grace unwitnessed among a civilized people, their loose and peculiarly formed garments assisting to set off their shape. A boddice (called Kanterie) fitting neatly to the form in front, reaches from the neck to the hip, conceals the bosom, but is left open behind; this with a gown (petia) fastened by a noose beneath the waist, and falling in loose folds to the feet, and a scarf (cadhi) thrown carelessly over the shoulder, completes their dress, which is made of cloth died with bright and varied colours. From their hair, and the tapes that bind their dress, are suspended long strings of courie shells, massive rings of silver clasp the ankles, and the arms, from the wrist to the shoulder, are loaded with broad rings of ivory, cut from the elephants' tusks, and dyed with varied dyes. The ceremonies attending the marriage of a widow are, as is usual among the natives of the east, few; the gift of a new cloth, and the selection of a fortunate hour on which to conduct the bride home, comprise the whole. With the young bride, a more lengthened rejoicing is made. On the marriage being assented to, the bridegroom pays one or two hundred rupees to the parents of the bride, and at the early part of the day, which the brahman who has been consulted has pronounced auspicious, two pyramids are constructed, by placing earthen pots one above another, ten or twelve feet apart, a bundle of firewood is laid behind each pyramid, and two wooden pestles, used by the women of every house in India to clean the grain, are planted perpendicularly between. The ceremonies last five days, during which the friends are feasted, the bride and bridegroom sitting on the ground between the pyramids, and on the fifth day, after being bathed by their respective male and female relations, the bridegroom leads to his tent his bride. The next morning the young wife rises early, and carrying the hand-mill near the feet of her husband's parents, there grinds the corn* necessary for the meals of the coming day, and is thus initiated into the practice of her domestic duties. The Binjarries are not restricted to one wife. It is rare, however, to have more than three or four in a house.

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* Shortly after midnight the women in the east rise and begin to grind corn for the family, cheering themselves in their lonely task by singing their labour songs. In several parts of Scripture this custom of grinding the corn for the day's consumption is noticed. "In the day when the grinders cease because they are few, and the doors be shut in the streets because the sound of the grinding is low. Ecc. xii. 3. and 4; See also Ex. xi. 5. and Is. xlvii. 1. where it says, 'Come down and sit in the dust, O virgin daughter of Babylon; sit on the ground; there is no throne, O

In the roving life they lead, exposed to the vicissitudes of a tropical climate, and liable to accidents and disease, we would fancy that necessity would have taught them some acquaintance with simples and the arts of life; but that custom, fatal to improvement, which obtains throughout India, binding each community to follow only those pursuits which their predecessors have been engaged in, prevails with equal effect among this migratory tribe, to whom every art is equally unknown. When sickness occurs, they lead the sick man to the feet of the bullock called "Hatadia," for, though they say they pay reverence to images, and that their religion is that of the Sikhs, followers of Nana Govind, the object of their worship is this "Hatadia," a bullock devoted to the god Balajee. On this animal no burden is ever laid; but decorated with streamers of red dyed silk and tinkling bells, with many brass chains and rings on the neck and feet, and strings of cowrie-shells and silken tassels, hanging in all directions, he moves steadily on at the head of the convoy, and the place he lies down on when tired, that they make their halting ground for the day; at his feet they make their vows when difficulties overtake them, and in illness, whether of themselves or cattle, they trust to his worship for a cure. This bullock is their god, their guide, and their physician.

From their migratory life, we are deprived of all means of calculating their numbers; but spread throughout the whole of India, in large bodies, they no doubt far exceed any amount of people which are brought to one individual's notice.

They bury the people who die unmarried, but the bodies of the married are burned. Food is placed at the head and foot of

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daughter of the Chaldeans, take the millstones and grind meal; and in Matt. xxiv. 41. it is said, 'two women shall be grinding at the mill, the one shall be taken and the other left.' One person can generally grind sufficient for the use of a small family, but where much is required, two women, as noticed in the Scripture, sit on the ground with the mill-stones between them.

the grave, but no omen of the state of the deceased is drawn from the creature that eats it.

HIRN-SHIKARRY OR HIRN-PARDY,—THE HUNTERS.

The Hirn-shikarry or Hirn-pardy, the Indian hunters, term themselves Bhourie. They are of short stature, greatly wanting in intelligence, and timid in their intercourse with their fellow men; while constant exposure to the vicissitudes of the seasons and their familiarity with toil and want, has stunted their growth and made them black and shrivelled in their form. Their numbers are great. They range from the snowy Himalayahs in the north through the vast plains of Hindustan, till at Cape Comorin, beneath the equator, the Indian ocean checks their further progress.* From each valley and each forest that civilized man has as yet left unoccupied, or has once again abandoned to the wild creatures of nature, the hunter obtains his means of subsistence. The creatures that they kill they eat, for, with the exception of the cow and bullock, all animals, the elephant, the tiger, and the leopard, the jungle dog and jungle cat, the wild-boar, the wolf, the iguana, and the rat and mouse, are used as food by the Bhourie. They obtain a little money by disposing of the skins of the animals they destroy, and often earn large rewards for destroying the leopards and wolves that at all times prowl about the outskirts of villages. The women, on visiting a town, gain a little money by disposing of charms and antidotes to the bite of a snake or scorpion's sting.

The language of the Bhourie seems to have little relation to that of any of the other migratory nations. It has many words like the Guzerattee and Mahrattee, and several of pure Sanscrit. The Bhourie are divided into five tribes, receiving among themselves the names, 1. Rhatore or Mewara; 2. Chowhone; 3. Sawundia; 4. Korbiar; and 5. Kodiara. It would appear that the hunters dwell in distinct localities, restrained from migrating to the hunting-grounds of other branches by custom and the fear of punishment, instances having lately occurred where the magistrate's authority has been called in to drive back tribes, who, urged by want, or enticed by more promising wilds, had quitted their own, and located themselves on the hunting-grounds of a neighbouring community.

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* Lt. De Butts, in his Rambles in Ceylon, describes a race termed "Veddahs," who, from his description, seem to be the same as the Bhouries of India.

These communities are governed by chiefs, termed "Howlia," who attain to their office by descent. It was difficult to obtain exact information regarding these head men; they would seem to be considered spiritual as well as civil guides, and among the wild untutored minds of these rude creatures, there seemed to be some vague idea that their Howlia is an incarnation of the deity. The occurrence of murder or other grave crime my informant had never heard of; but all minor matters are decided by these chiefs. On them likewise devolves the duty of summoning the different members of the tribe to aid in snaring the tiger, for which villagers and proprietors occasionally offer high rewards. This, when earned, they divide into three shares, one for the god of the river, one for the god of the wilds, the remaining third being apportioned equally among those who were present at the capture, the Howlia or chief obtaining no greater sum than another of the community. They all assemble at the Holi festival, at the place of the Houlia's residence, when he collects his income, the community subscribing one rupee a head.

Among other modes of obtaining subsistence, thieving is one which they look to as no small means of support. Gang robbery, or any system attended with violence, they are not addicted to; but no field or stack of grain is safe from their depredations when they are in the neighbourhood. For this, severe fines, and death itself, were often inflicted on them, while the country was ruled by the native princes; for though the hunters have only a narrow loin-cloth as clothing, and the persons of the women are scarcely hidden by the few rags they pick up in the fields and sew together, yet, when in the grasp of native chiefs, the fear of death has made them produce two to five thousand rupees to purchase forgiveness and regain their freedom. It may be from the recollection of such scenes, that, notwithstanding their seeming poverty, all classes assert these wretched-looking beings to be the possessors of vast wealth, and, when in the fields in their lonely camps, sheltered by a few tattered rags stretched overhead, they are at intervals plundered by the ruthless robbers we term decoits.

For the first five years after the beard first appears, it and the hair is cut once a year, but ever after they wear both unshorn, and their long shaggy locks add to their uncouth appearance. The bodies of the dead are buried. Few attain sixty years of age, and ten is the greatest number of children they have known one woman to bear; nor have they ever heard of any one being killed by a tiger, though one of them has assisted at the capture of eight of these creatures. They call themselves a branch of the Dhoongur, the Shepherd or Vesya race.

THE TAREMOOK, OR WANDERING BLACKSMITH.

The Wandering Blacksmith is known in the Dekhani language, as Ghissaris; as Lohars by the Mahrattas; and from the Canarese they receive the name of Bail-Kumbar, but they term themselves Taremook.

Their traditions affirm the northern provinces of Hindustan to have been their original country; but the cause or the period of their emigrating thence has not been preserved. As a race, they are dark, though not black, and somewhat taller than Hindoos in general. They are to be seen dwelling on the outskirts of almost every village throughout India, though their numbers are not great; the largest number of families the old Taremook who gives me this information has ever seen in one place, amounting to ten, a community of perhaps sixty people. It is rare to find them occupying houses in towns; but, for the greater facility of migrating, they encamp outside the walls, where they reside, exposed to the changes of the weather, from which they are barely sheltered; a ragged and patched cloth, two or three yards long, being all a family have for their protection. They are blacksmiths by trade, and are very poor, living from hand to mouth. The women collect wood in the jungles, to make the charcoal necessary in their husbands' trade: the movement of the forge-bellows is likewise the duty of the women, many of whom assist their husbands by working the sledge-hammer. Their language they term Taremooki: that spoken by the communities in the Dekhan contains several Mahrattee and Canaree words, a mixture probably resulting from their lengthened sojourn on the border countries of these two nations.

The richest Taremook my informant has ever seen, was said to be worth ten thousand rupees; but though some individuals collect a little money, he has never known any one learn to read or write. The dress of this migratory race is like that of other Hindus. Their religion is the Brahminical, Kandoba being the deity to which their worship is chiefly directed. Their marriages are conducted similarly to the customs of the Hindoos, but intoxicating drinks are largely used. They have earned a great name for gallantry, and it is a very usual thing to hear of the rough Taremook levanting with another man's wife. On the occasion of a birth, they sacrifice in the name of Satwai. They burn the bodies of married people, and lay the ashes by a river's side; but the unmarried dead are buried, and for three days after the funeral food is carried to the grave, though they draw no augury of the state of the soul of the deceased from any creature eating the food.

THE KORAWA.

This migratory people arrange themselves into four divisions, the Bajantri, Teling, Kolla, and Soli Korawas, speaking the same language, but none of them intermarrying or eating with each other. Whence they originally migrated it would be difficult perhaps now to come to a conclusion, nor could it be correctly ascertained how far they extend. The Bajantri or Gaon ka Korawa, the musical or village Korawa, are met with in Bejapore, Bellary, Hyderabad, and throughout Canara. The men of this people are somewhat more robustly formed than the settled population; but the females are less tall, and more dark than the Canarese women among whom they are located. Their food differs from that of the Hindoo as well as the Mahomedan; they never eat the cow or bullock, but the jackal, porcupine, hog and wild boar, deer and tigers, are sought after and used by them. They deny that robbery is ever made a regular mode of earning a subsistence; an honesty, however, that the people among whom they dwell give them but little credit for. Indeed, from my own observation, on an occasion that brought the circumstances of a community to the light, it is difficult to believe that the great sums found in their possession could have been honestly earned. They live by thieving, making grass screens and baskets. The men likewise attend at festivals, marriages, and births, as musicians, which has obtained for them the name of Bajantri; and at the reaping season all resort to the fields to beg and pilfer from the farmers, for they will not be induced to put their hands to labour. The women, too, earn a little money by tattooing on the skin the marks and figures of the gods, which the females of all castes of Hindus ornament their arms and foreheads with. The Bajantri korawa reside in mud huts, in small societies outside the walls of the village to which they have temporarily attached themselves. The age for marrying is not a fixed time; and, different from every other people in India, the youth of the female is not thought of consequence, the old man telling this when a lad with mustaches just appearing, having been married to a woman who, five years previously, had attained maturity; a marriage that would have been opposed to the customs and repugnant to the feelings alike of Hindoo and Mahomedan. To this wife he yet remains attached, though it is not unusual to have two, three, or four wives in one household, among this people. In marrying, at the hour pronounced to be fortunate by a Brahmin, the bride and bridegroom, smeared with turmeric, are seated on the ground, and a circle drawn with rice around them. For five days the musicians attend before their door, and the whole concludes by the neighbours gathering round and sprinkling a few grains from the rice circle over the couple. The married women wear the tali round their necks, which is broken on the husband's death by the relatives of the deceased. This people live virtuously; the abandonment of their daughters is never made a trade of, and other classes speak favourably of their chastity.

They respect Brahmins; and though they never, or at least very rarely, attend places of worship, they seem to respect the gods of the Hindoo mythology, and keep in their houses small silver images of Hanuman, which they once every two or three months worship with songs, and sacrifice, and music. Their foreheads, too, are tattooed with the mark of Vishnu; but they offer up no daily prayers.

THE TELING KORAWA, OR KORAWA OF TELINGANA.

This branch of the Korawa people are generally known as Kusbi, Korawa, Aghare Pal Walé, prostitute Korawas, the sitters at the doors of their tents; but these names the people themselves consider opprobrious. The form of their features is altogether different from that of the Bajantri Korawa, the shape and expression of the countenance being similar to the inhabitants of the Coromandel coast—the country, if we judge by their name, Teling, whence they originally migrated: but wandering from place to place for a livelihood, wherever the Madras troops marched under Sir Arthur Wellesley, they followed, and are now found located in most British cantonments. The Teling Korawa gain a livelihood by basket-making and selling brooms, in making which their wives assist; but their chief means of subsistence is in the prostitution of their female relatives, whom, for that purpose, they devote to the gods from their birth.

When the lives of children in India are despaired of, the fond mother, whether Mahomedan or Hindu, wills that it should live, though sickness and destitution be its lot through life; and when agonized by the prospect of its death, she vows to devote her offspring to the service of the deity, should its life be spared. With the Mahomedans, the male children thus devoted become durveshes, and their females termed 'Mustanis,' attach themselves to one or other of the four large communities of Fakirs, who beg in India, the Mustanis being supposed to live a life of virtue. Among the Hindus, again, there are two classes of devoted women, the one attending the temples and living a life of chastity, the other class fulfilling the vows of their relatives, by promiscuously sacrificing to sensual love. The Brahmins, who, worshipping a deity generally as pure theists, whether followers of Brahmna, Vishnu, or Siva, are seldom guilty of thus throwing their females on society; and this practice seldom obtains among the better classes of Hindus even. But as this pursuit of the women thus devoted, however public it may be, entails no disgrace upon the women themselves, or their families, many of the low castes and migratory tribes of the Hindus have readily taken to a practice which allows them to follow a profitable calling, without suffering in the opinion of their neighbours; and as the poorest and most wretched community in India attach the utmost importance to the purity and conjugal fidelity of their unmarried and married females, the low castes and outcasts to whom money offers a great temptation, devote their female children in their earliest infancy, and thus are able to practise their profession without restraint.

The goddess, in whose service the lives of the Teling Korawas' devoted women are thus to be spent, has her chief shrine near Bellary. They never devote more than one of their daughters; the rest are married and made honest women of. The devoted women, notwithstanding their loose lives, occasionly bear children, so many as four having been the children of one mother. These children are treated as if legitimate, being admitted without purchase to all the rights and privileges of the caste. It is probably owing to this intermixture the varied colours we find among them arise, changing in individuals from the fairness of the Brahmin to that of the darkest coloured Sudra.

They have no rules or laws among their community for self-government. They eat the deer, the hare, and the goat; but the cow is considered a sacred, and the hog an accursed animal, and never used as food. No one can read or write.

They are very rarely allowed to reside inside towns; but when this liberty is granted them, they pitch their tents or erect grass huts at a distance from the dwellings of respectable people. The women wear a boddice (choli) open in front, and a sarhi; the men dress as Hindus usually do.

This branch bury their dead, and the food that was most liked by the deceased is placed at the head of the grave. The most favourable omen of the state of the departed soul is drawn from its being eaten by a crow; less auspicious if by a cow; but if both the crow and cow decline to eat it, they deem the dead to have lived a very depraved life, and impose a heavy fine on his relatives for having permitted such evil ways.

Their religion is the brahminical, and Brahmins assist at all their ceremonies. Their language is nearly similar to that spoken by the Bajantri Korawa, with whom they agree in the arrangement of the Korawas into four branches. The other two, in addition to the Bajantri and Teling Korawa, I never met in with. They are called Koonsi Korawa, and the Patra Korawa, or Patr Pulloo. Their manners and habits and mode of life are scarcely dissimilar from one another; all of them can converse in their own language, but they do not eat or marry with an individual of a different branch.

THE BHATOO.

This migratory people are known in India by the name of Doomur or Kollati. They are spread over the whole of the great continent; but though retaining among themselves the name of Bhatoo, they are arranged into several distinct tribes, speaking different tongues, and holding no intercourse with each other. One of these tribes occupy the country from Ahmednuggur in the north, to Hurryhur in the south, and lie between Bellary and the western shores of India.

The Bhatoo are seldom tall, rarely exceeding five feet two inches in height, and the women attaining a proportionate size.

At the period of adolescence, however, the young men and women are perfect models for the sculptor, the plumpness of that age rounding off the form, and hiding the projecting bones and the hollows between the muscles, which, in after life, the profession that both sexes follow too prominently develop. They are "Athletæ;" and the boys and girls are trained to the most surprising feats of agility from their earliest infancy. Besides this, which is their ostensible mode of gaining a livelihood, the men of this wandering people earn sums of money by exorcising demons from the persons of those they possess;* but what they most trust to for support is devoting their female relatives to the gods.

The various castes of Hindus have their various gods, at whose shrines the children are devoted; but the god of this Bhatoo is Kandoba,† in the village of Jeejoorie, near Poona. About the age of five they carry their female relations there, and after performing sacrifice, and burning frankincense, they lay the girl at the feet of the deity, to which she is now considered married. These devoted women, and all the male children, are regularly trained to athletic exercises, and the community wanders from village to village to exhibit. Most of their feats are performed by means of a bamboo. On the morning of the day they intend exhibiting, they abstain from all food, and to this rule they attribute much of their freedom from disease; and my informant, an old man sixty years of age, can recollect no instance of rupture among them. Before his own eyes, however, he has seen four people killed by falls from the bamboo, innumerable injuries sustained by others, and he himself has his right elbow joint fearfully crushed.

They settle unimportant points among themselves by arbitration, but all serious matters are brought for the decision of their British rulers. They are totally uneducated; the old man giving me this information has never seen or heard of any one who could read or write. Impressed with the belief, prevalent throughout India, that the muscular system does not retain its vigour after marriage, the Doomur or Bhatoo delays marrying till middle-aged; and then, owing to the great expense the ceremonies when taking a young wife occasion, the Bhatoo usually allies himself with a woman who, having been devoted to the gods in her infancy, has now become too old to make a trade of her charms, and too stiff to take a part in the athletic exhibitions. Two or three hundred rupees are expended in marrying a young wife; but the ceremonies for the older women are completed in a day, and cost only ten or twelve rupees. Yet, notwithstanding this mode of life, they are not unprolific, my informant having seen five, six, seven, and even eight children born of one woman who had been devoted in her infancy to the gods.

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* Insane people are frequently taken to have the demon cast forth to these people, and are occasionally placed in a cleft of a tree,—these, of course, are not benefitted by the processes, but demons are frequently cast out of people who had no demons in them.

† An incarnation of Mahadeva.

They never eat the hog, the cow, the bullock, or the horse. They call themselves Mahrattas, but their religion seems essentially different from the Hindus around them. They own attachment to none of the three great divisions of the brahminical faith, and when asked whom they worship, they reply, "Narayan," the Spirit of God; but the particular object the Bhatoo pays his devotions to is the bamboo, with which all their feats are performed. At the village of Thekoor, near Kittoor, the shrine of the goddess Karewa has been erected on the summit of a hill, around the base of which dense forests of bamboo grow. One they select, and the attendants of the temple consecrate it. It is now called "Gunnichari," Chief, and receives their worship annually. To it, as to a human chief, all respect is shewn; and in cases of marriage, of disputes requiring arbitration, or the occurrence of knotty points demanding consultation, the gunnichari is erected in the midst of the counsellors or arbiters, and all prostrate themselves to it before commencing the discussion of the subject before them. The Bhatoos do not keep idols.

All the dead are buried; when they consign one of their people to the earth, they place rice and oil at the head of the grave, and stand near to watch what creature comes to eat it, drawing the happiest omen of the state of the departed from the crow visiting the spot.

THE MUDDIKPOR.

Many names have been given to the migratory people we are now noticing; Koeli Katr, or Kootaboo, Kublgira or ferryman, Koli, and Barkur, are those most usually employed; but Muddikpor is the designation they apply to themselves, They are generally tall and powerful men, with an olive-yellow complexion, and are now very numerous throughout India. They say that their original locality was the village of Talicot, near Sorapore, and that however far they be now dispersed, all classes continue to speak the Mahratta tongue, though they must likewise acquire a knowledge of the language of the country they wander about in, to enable them to earn a livelihood. Their traditions carry back their origin to the obscure periods of Hindu history; and they say they have sprung from ten individuals, and thus account for the ten tribes into which we now find them divided; and this traditionary account of a common origin receives corroboration from the circumstance that all the tribes marry and eat together.

In each tribe an individual is superior to the others, to whom the rank descends by birth, though no title is attached to the office. All disputes that arise are arranged by a jury, whose decisions are made in accordance with the customs of their forefathers received by tradition.

These wanderers earn a living by catching fish with nets, and their women earn a little by knitting, and by tattooing the dark blue marks on the foreheads of the brahmins and lingaets; but their chief occupation is the exhibition of the transparencies used in representing the battles of the Panch Pandya, five brothers, whose exploits are, we believe, detailed in the Ramayana. The figures are painted on deer-skin with very brilliant colours, and the story being one the Hindu never tires in listening to, in every village after night-fall you may see the representations of the battles, and hear the Keeli Katr describing the heroes' deeds.

Their females are very virtuous, and one woman has been known to give birth to twelve children. Reading and writing is unknown among them. Their dress and food are the same as the Hindus among whom they dwell.

They live in square huts formed of grass sewed together, the whole being perhaps a rupee in value. These they themselves make and carry with them at their periodical migrations, which custom renders obligatory every three months,—a longer stay would, they say, subject them to some dire calamity; and as the third moon passes by, the spot that yesterday was a merry encamping ground is to-day a desolate and unoccupied waste.

The Muddikpor seemed to me to have no idea of a supreme being. They pay their devotions to the transparent figures with which the battles of the Panch Pandya are represented: the box of bamboo containing them is each morning placed on a part of the floor fresh covered with cow dung; and on the lid being opened to expose the drawings, they burn frankincense, and bow down to the ground in worship,—"Oh Panch Pandya, by you we live, continue to give us our daily bread!"

They are not restricted to one wife, and they bury all their dead, except lepers, whom they burn.

The languages spoken by these tribes are not understood by any one of a tribe different from their own, though there seems a general similarity among them, as will be seen from the few words I obtained. The Sanscrit, Tamil, Telagoo, Guzerattee, and Maharattee, have been placed to enable a comparison to be made.

ENGLISH.	SANSCRIT.	TAMIL.	TELAGOO.	GUZERATTEE.	MHARATTEE.	GOHURIE.	BOWRIE.	TAREMOOKEE.	KORAWAEE.	BHATOOEE.
Earth	Bhoomé	Bhoomi	Bhooné	Zameen	Poortooi	Jamee	Bhoé	Mattri	Tirri	Bhooé
Stone	Pashan	Kulloo	Rai	Pathar	Duggr	Bhatta	Bhattoo	Duggroo	Kellay	Pathar
Water	Ap. Ootk.	Tanni	Neel	Pani	Pani	Pani	Pani	Pani	. . . .	Pani
River	Naddi	Ar	Eeroo	Nuddi	Nuddi	Nandie	Nandie	Nudd	'Ar	Nai
Tree	Vrikoh	Marm	Chet'	Jharr	Dzar	Jharr	Jhar	Jharr	Mooroo	Jhar
Bread	Bhoojn	Roti	Ruttie	Rotli	Bhakrie	Bhatta	Rutto	Bhatur	Retti	Ruttie
Sunshine	Ooshun	Vayil	Eenda	Tirkhoom	Nimber	Turko	Taoro	Turko	Wuggul	Ghum
Fire	Agni	Nerpu	Nepu	Attăs	Vestoo	. . . .	. . . .	. . . .	Nerpu	Ugg
Wind	Waioo	Kath	Gali	Paon	Warra	Warr	Waero	Waroo	Kas	Bara
Deer	Mirg	Maun	. . . .	Hirn	Hirn	Kuryar	Hirn	Hirn	Chigree	Hirn
Man	Manish	Mansum	Mansi	Manus	Manus	Gohur	Mankhoe	Lokro	Amloom	Moons
Woman	Streea	Pomli	Armansi	Baidi	Baikoe	Gohurni	Manussi	Chali	Punjeri	Jo
Husband	Rutti	Amli	Pemli	Mati	Navra	Gohur	Bhowrie	Bhoyé	Mánaga	Matoe
Wife	Stree	Pundati	Pemloom	Bairri	Baiko	Gohurni	Bawun	Baiko	Koolsi	Bako Kudjia
Boy	Balig	Payun	Kurrur	Chokro	Chokra	Chora	Chora	Porwyo	Amlamoo	Chora
Girl	Cunya	Sirki	Ar Pella	Chokrie	Chokrie	Chorie	Chorie	Lokri	Hena Maga	Chorie
Son	Shuphoo	Magn	Kurpoe	Dekro	Chokra	Chora	Cheea	Beto	Amlamoo	Chora
Daughter	Kunna	Maga	Kootroo	Dekrie	Chokrie	Chorie	Cheei	Porni	Magga	Bittie
Father	Peeta	Appin	Tinree	Bawa	Bapp	Bap	Baoo	Aoo	Aoo	Bappo
Mother	Mata	Amma	Amma	Mairee	Ai	Yaree	Aya	Ai	Amma	Yajee Amma
Bull	Virishab	Maroo	Eeddoo	Dorr. Burrod	Bail	Bullog	Dhando	Bail	Marr	Bail
Cow	Gao	Pussoo	Ao	Gae	Gae	Gowrie	Gai	Gai	Akl	Gai
Horse	Ashp	Coodrie	Gurrum	Ghora	Ghoro	Ghora	Ghoro	Ghoro	Coodrie	Ghora
Mare	. . . .	Putta Coodrie	Ar Gurrum	Ghorie	Ghore	Ghorie	Ghorie	Ghorie	. . . .	Ghorie
Dog	Shwan	Nai	Kookka	Kuttro	Kuttra	Kuttra	Kuttreo	Kuttro	Nai	Kutta
Bitch	. . . .	Putta Nai	Ar Kookka	Kuttie	Kuttrie	Kuttrie	Kuttri	Kuttri	Putta Nai	Kuttie
Boar	Sookur	Punni	Moga Pendi	Dookr	Dookr	Soor	Koor	Dookr	Pandee	Tunda
Cat	Sookri	Poni	Pellee	Billari	Manjan	Bălăi	Billé	Mandur	. . . .	. . . .
Cock	Koorkoot	Chawul	Poonzoo	Murgh. Kokur	Koomra	Kukro	Kokro	Kokro	Korla	Kukur
Hen	Koorkooti	Kolie	Korie	Kokri	Komri	Kukrie	Kokrie	Kokrie	Korlie	Kukun
Duck	Budhag	Bāt'h'h	Bāt'h'h	Butah	Batto	Budah	Budhug	Budhug	. . . .	Budah
Tiger	Wiagr	Peeli	Peeda Pooli	Bag. Gaj	Wag	Bag	Wag	Wag	. . . .	Bag
Goat	Ajja	Arroo	Meka	Buckra	Bokr	Cheela	Bokro	Bokro	Mekur	Buckra
Sheep	Ajja	Koorm Arroo	Goorie	Menda	Meenda	Meenda	Gadri	Mhenda	Goorell	Mhendee
Jackall	Jambook	Narri	Nakka	Kola	Kola	Salia	Nohrie	Koloo	. . . .	. . . .
Birth	Junnunkar	Pergalm	Kantadi	. . . .	Prassut	Wokin	. . . .	Jannas	. . . .	. . . .
Marriage	Bewaha	Kalyanam	Penli	Lagan	Lagan	Beha	. . . .	Hurwo	. . . .	. . . .

English.	Sanscrit.	Tamil.	Telagoo.	Guzerattee.	Mharattee.	Gohurie.	Bowrie.	Taremookee.	Korawaee.	Bhatooee.
Death	Mritthoo	Saogalam	Sao	Mouat	Mella	Murgeo	. . . .	Murĭgo	. . . .	. . . .
Hot	Ooshum	Soorra	Oorgoo	Oonoo	Wasnoo	Tattoe	Oon	Garm	Suralla	Tatta
Cold	Thaud	Arootoo	Saldi	Tharoo	Thand	Sheela	Seetul	Thad	Thand	Sheela
Great	Praood	Pirrs	Pedadi	Mothnoo	Mota	Muthoe	Wadial	Mato	. . . .	Muthœ
Small	Lahan	Chinnada	Chinadi	Nana	Lahan	Nankia	N'hānū	Nahango	. . . .	Nankia
Bad	Wyte	Katadoo	Chidadi	Nakaroo	Wyte	Naswie	Khrab	Khrab	Narad Ketso	Sarya
Good	Samichenum	Nalado	Munchadi	Saroo	Changla	Acho	Achewat	Bess.Changloo	Nalado	Nikko
Quick	Tewra	Shigrum	Tirri	Ootawa	Jaldie	Judkurdie	Mullie	. . . .	Méné	Juldee Bhigi
Slow	Shunni	Mulloom	Medligoo	Hurwé	Halloo	Hullia	Hullo	. . . .	Logo	Hulloo
Hunger	Shuddha	Pussi	Akkli	Bookh	Bookh	. . . .	. . . .	Bookh	. . . .	. . . .
Thirst	Trish	Tawum	Dupaki	Tirs	Piass	Tursi	. . . .	Tars	. . . .	. . . .
Calf	Watso	Kunnoo Oti	Doora	Wachroe	Wasroe	Kera	Keldie	Wasro	Kero Kuttie	Parra
Hare	Sussar	Mossul	Koondel	Sussoo	Sussa	Sussia	Dhantrie	Susroe	Kundelli	Sussa
Ass, Male	Garho	Kalide	Garrdé	Gudhairoo	Garrhoo	Gudha	Gudhanoo	Gadhroo	. . . .	Gudha
— Female	. . . .	Putta Kalidé	Ar Garrdé	Gudhairee	Garrhwi	Gudhie	Gudhairie	Gudhri	. . . .	Gudhie
Kite	Ghar	Prandu	Gedda	Khalell	Ghar	Sumlie	Khumlié	Ghar	Perzah	Ghar
Crow	Kag	Kaka	Kaki	Kagra	Kaora	Kagla	Kogroo	Kooroo	. . . .	Kasla
Shoe	Jora	Papoos	Papoos	Joro	Jora	Kasra	Chumpalé	Joro	Papoos	. . . .
Sarhi	Wastra	Porawé	Koka	Sarhi	Lugra	Sarhi	Khari	Lugroo	Selhi	. . . .
Sword	Seelita	Katti	Ketti	Turwar	Turwar	Turwar	Turwar	Tirwar	Ketti	Turwar
Black	Krisha	Karp	Nalpo	Karo	Kalung	Kaloo	Kajuloo	Kala	Kart	Kala
White	Shoobra	Virlé	Telkoo	Oojroo	Saped	. . . .	. . . .	. . . .	Wulla	Dhowra
Red	Ratawai	Saipoo	Eeer'pu	Ratho	Tamburum	Rathro	Khoondur	Lall	. . . .	Lall
Yellow	Petawar	Manjoo	Patsadi	Peloo	Peola	Pela	. . . .	Puloe	. . . .	Peela
Green	Heerwa	Hitché	Pasroo	. . . .	Heerwa	. . . .	. . . .	. . . .	Pusroe	Herwa
Blue	. . . .	Neelum	Nelpoo	. . . .	Parwa	. . . .	. . . .	. . . .	. . . .	Nela
Wheat	Ghodoon	Godmi	Godmalloo	Goun	Goun	Goehoon	Gohoon	Gāhoo	Godmi	Gehoon
— Flour	Choorn	Man	Pindi	Attoe	Kuneek	Attoe	Lote	Peet	Man	Atta
Grinding stone	Ghirt	Yendrum	Terigalay	Ghatti	Ghirtee	. . . .	Ghiltrie	Ghirti	. . . .	Chickie
Head	Kuppul	Talli	Talla	Matho	Dooka	Mathoe	Goddo	Mathoe	. . . .	Moondhi
Eye	Doli	Kunnoo	Kanloo	Ankh	Dora	Ankhi	Dolo	Dolo	. . . .	Akhœ
Nose	Nashik	Mook	Mookka	Nak	Nak	Nak	Nak	Nak	. . . .	Luk
Ear	Karn	Kaddoo	Chouloo	Kan	Kan	Kan	Kan	Kan	. . . .	Kunnoo
Hand	Hast	Kaiee	Chai	Hat	Hath	Hath	Hatha	Hath	. . . .	Hut
Foot	Pa	Kall	Kalloo	Pug	Pae	Pae	. . . .	Pug	. . . .	Pae
Thumb	Ankta	Peri Virlm	Botnell	Angatoe	Angotha	Angotha	Bote	Angoto	. . . .	Bode

Description of an Improved Water Meter, invented by Mr Alexander Mitchell, Watch and Clock Maker, Glasgow. By JAMES THOMSON, Esq., F.R.S.E., M.R.I.A., F.R.S.S.A., Civil Engineer, Glasgow. (With a plate.) Communicated by the Royal Scottish Society of Arts.*

THE action of the meter, referring to Plate I., may be described as follows :—

The supply-pipe *f f* being connected to the water main by a coupling, &c., in the usual way, the water flows through the valve *g*, and passing into the cylinder *h h h h* at *j*, is discharged by the pipe *k* into the receiving cistern *l l*, from which the water is drawn off by a stop-cock *l'*.

Upon one end of the axis or spindle of the screw *a a* is fitted a pinion *d*, working into the train of wheels *e e e e*, so arranged as to indicate the quantities of water discharged either in gallons or cubic feet, similar to the index of gas meters—the whole being set in motion by the flow of water through the cylinder acting upon and causing to revolve the Archimedean screw enclosed within it. To prevent the screw from being driven forward by the water, and in order to reduce the friction to a minimum, the end of the axis at *d* has a hard steel point inserted, which works in a cock or pottance fitted outside, and enclosed in an oil box, supplied with oil through the filler *d'*.

In order to render the indications of the meter uniformly correct, under different heads or pressures of water, the following very ingenious method of adjustment is adopted :— Upon the axis of the screw are fixed four thin brass wings or leaves *c c c c*, each moving upon a separate hinge or pivot *c'*, fixed at right angles to the axis, with screw-nuts upon the end of each pivot, by means of which the wings can be maintained open or shut at pleasure, and so lessen or increase the discharge of water at each revolution of the screw. The lower pair of wings in the drawing are represented slightly open. With this power of adjustment, it is very easy to regulate the quantity of water discharged under different pressures, so as to correspond exactly with the index and train of wheels, which can best be ascertained by trial upon setting up the meter, say by a measurement of 20 gallons, and opening or closing the fly leaves, to bring the *quantity measured and the index to correspond.*

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* Read before the Royal Scottish Society of Arts, 12th Dec. 1842.

It will be evident that the quantities of water passing through a meter, upon this principle, would be correctly indicated down to that quantity requisite to overcome the friction of the screw, below which amount, however, the water would pass through the cylinder without affecting the index, and consequently without being registered. Although the quantity thus passing would not be considerable in a well-constructed meter, Mr Mitchell has recently introduced an improvement which completely obviates this objection, and renders the indications correct under any circumstances, and down to the smallest quantity.

This is accomplished by means of the conical valve cock *g*, which is so constructed as to act instantaneously, and so keep the supply either flowing at the full bore, or suddenly shut off, when the cistern is full. The opening and shutting of this valve is effected in the following manner:—To the end of the lever *h h*, which works upon a journal, and is raised or depressed by the ball-float *i*, are attached the two arms *p p* and *o o*. As the water in the cistern rises, these arms are carried forward without acting, however, upon the valve, until one of the chains, connecting them with the lever and lead-weight *n n* is upon the stretch. At this point the lever and weight are upon a balance, so that the least further rise of water in the cistern carries it suddenly over, and allows the valve *g* to close. A reverse motion takes place on the fall of the cistern water, the action of the other arm and chain *p p* opening the valve, which a small catch retains in its place against the force of the supply water. To prevent any injury to the pipes from the sudden action of the valve, an air-vessel *m* is attached to the supply-pipe immediately above the position of the valve.

The introduction of this improvement, which prevents the possibility of water passing unregistered through the cylinder, renders this description of meter very perfect, and capable of indicating accurately the smallest as well as the largest quantities. From the small cost, too, at which they can be manufactured, it is to be hoped they will soon be brought into general use, and *substituted for the present unequal mode of water assessment upon house rent.*

Reference to the Plate.

A B C D. Outer case of thin cast iron for enclosing the apparatus shewn in section.

a a. Archimedean screw working in the cylinder *b b.*

c c. Thin brass wings or flys, with adjusting screws for regulating the motion of the screw.

d. Pinion upon the end of the screw-spindle or axis.

e e e. Train of wheels set in motion by the pinion *d,* to indicate the quantity passed through the cylinder, closed in with a glass front.

f. Supply pipe.

g. Valve worked by the lever *h h,* connected to the ball float *i.*

h h. Lever for opening and shutting the valve *g.*

k. Discharge-pipe flowing into the cistern *l l.*

m. Air vessel to prevent injuring the pipe on opening and shutting of the valve.

n n. Lever, with lead to retain the valve open or shut.

o o and *p p.* Arms fixed upon the end of the lever *h h,* and attached with chains to the lead-weight *n.*

REPORT OF COMMITTEE.

Your Committee having met with Mr Mitchell, and having again heard his explanations, and carefully examined the water meter submitted to them, came to the following conclusions:—

1. That as far as they can judge, this meter may be considered sufficiently accurate to form a fair measure between water companies and their customers.

2. That its construction is simple, and well devised for permanent practical use, being little subject to derangement from the wear of its parts.

3. That its rate of registration being susceptible of easy adjustment, either in plus or minus degree, and its actual delivery, during certain portions of time, being always ascertainable by the consumer, its use may be adopted with confidence, as being alike equitable to the suppliers and the consumers.

All which is respectfully reported by

THOS. GREIG, *Convr.*

13th December 1842.

Description, with Drawing, of a Self-Acting Stopper for Winding-Engines. By Mr John Maxton, F.R.S.S.A., Engineer, late of Greenock, now of Leith. (With a Plate.) Communicated by John Scott Russell, M.A., F.R.S.E., F.R.S.S.A.* Communicated by the Royal Scottish Society of Arts.

My attention having been directed to the numerous accidents which occur in consequence of Winding-engines at coal-pits being left in charge of careless engine-men, I designed the apparatus, represented in Plate II, to be attached to the gearing of the engine, so that, independent of the engine-man, the machinery would stop when the tub or bucket had reached its proper height.

In fig 1, A is the fly-wheel shaft of engine, on which is fixed a bevelled pinion B, which works the wheel C, into the eye of this wheel a screwed rod J moves, having an oblong hole D at the opposite end; V is the eccentric rod, E is the double hand-gear crank, O is part of the fly-wheel, *p* is a friction-strap, F is a lever with a heavy ball of metal on the upper end, G is a link connected to the eccentric rod. In this link a pin, fixed in a lever H, is made to move freely, so that when the edge of the oblong hole D comes in contact with the ball-lever F, it will throw it either way, according to the direction in which the engine and screwed rod J moves; when the lever F falls, it will tighten the friction-strap on fly-wheel, and throw the eccentric rod out of gear; by the pin in the lever H acting on the top, or under side of link G. The reason why the link is long, is to allow the crank and pin H to travel (by the screwed rod in wheel), without moving the eccentric rod out of gear, till it arrives at either top or bottom of link, which is the proper time that the engine should be stopped, the tubs being then at the proper height. A handle is attached to the shaft K, for the purpose of allowing the engine-man to take the strain off the friction-strap to let the engine get under way. Figs. 2,

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* Read before the Royal Scottish Society of Arts 24th January 1842. Report of Committee read and approved 13th June 1842, and the Society's Silver Medal, value Ten Sovereigns, awarded 14th November 1842.

3, and 4, shew the apparatus in its different positions. The estimated cost of the whole is about 20s. per horse-power.

JOHN MAXTON.

GREENOCK, 12*th Nov.* 1841.

Report of Committee of the Royal Scottish Society of Arts on Mr Maxton's Stopper for Winding-engines.

We have carefully examined and considered the paper and drawings illustrative of Mr John Maxton's invention, for the purpose of preventing the accidents arising in collieries from the *over-winding* of the ropes by which the coals, &c. are raised from the pit, and we have to report as follows:—

First, The apparatus has not, we believe, been yet tried, and in so far as we can form an opinion, without having seen it in actual operation, we consider that there is no practical difficulty to prevent its answering the purpose intended; and,

Secondly, We are of opinion that much ingenuity has been displayed by Mr Maxton in designing the mechanical arrangements by which the object in view is proposed to be effected.

DAVID STEVENSON.
HUGH MORTON.
JOHN MACKIE.

EDINBURGH, 26*th May* 1842.

Addendum.

In heavy winding-engines it is necessary to decrease the quantity of steam as the tub approaches the surface. I, therefore, think it would be necessary to have a connection betwixt the apparatus and the throttle-valve of the engine, so that the steam could be cut off at the same time that the eccentric rod is thrown out of gear; and this I think could be very easily effected.

JOHN MACKIE, *Mining-Engineer.*

ALLOA, 31*st May* 1842.

Postscript to Description of Mr Tait's *Portable Diorama, which may be viewed by a number of persons at a time; on page* 275 *of the preceding volume.* Communicated by the Royal Scottish Society of Arts.*

The quantity of extraneous light seen by the spectators, under the arrangement described in that article, is much diminished, and the effect produced is proportionably improved, by placing across the board in front of the pictures, a parapet covered with black velvet, as high as the bottom of the pictures, and at such distance from them as to allow the stray rays from the front light to fall behind it; and by covering the whole of the upper surface of the board, in front of the parapet, with black velvet, removing the check-pins to the *side* of the board.

G. T.

Edinburgh, 17*th April* 1843.

Some Remarks on the Methods in common use of obtaining the Mean Temperature of Places, and on the supposed difference between the Temperature of the Air and that of the Earth. By Professor W. M. Carpenter.

It is stated by Humboldt and others, that the mean temperature of the coldest springs in warm climates, is often lower than that of the air of the same places. If we examine those agencies in which atmospheric temperature originates, and by which terrestrial temperature is modified, we shall perceive that such a condition could not exist, and consequently, that the observations on which such conclusions were based, were not accurate, or that some unsuspected agency must modify the relations which should otherwise be constant. In examining the meteorological records of our own country and of other parts, and comparing the observations made by different persons resident at the same places, we shall perceive that the results differ not more from the mean temperature of the place, than from each other. These discrepancies, or rather these departures from accurate results, are dependent on many circumstances. In the first place, thermometers of very inferior quality are in very general use, and they will often differ in the results afforded by four or five degrees. In the next place, sufficient importance is not attached to the position of the instrument; and instead of having it completely protected from reflection, and in almost complete obscurity, taking care at same time to secure a free circulation of air about it, we find them often in such a situation as to receive from walls or the earth, a considerable portion of reflected heat; often in closed apartments or against walls, the temperature of which is influenced throughout the day, by the full force of the sun. Galleries fronting the north are favourite places for suspending thermometers, under the impression that no heat is reflected from that side. In this way we often find, in this climate (the United States), that when a thermometer properly placed gives a temperature of 90° or 91°, all the others in the vicinity will stand at 95°, 100°, or even 110°; so that the annual mean derived would be very greatly above the true one. There is, however, it appears to me, a source of error much more general. I speak of the methods of calculating the mean after the observations have been made, and of the adoption into general use of methods which have been found to give correct results at particular places. The methods most in use are the following, viz., to take three observations, and from these calculate the mean directly; some fix upon sunrise, 2 P.M., and sunset; others, as in the Army Meteorological Register, and in those of most of the meteorological societies, upon 7 A.M, 2 P.M., and 9 P.M. I have ascertained, by taking the mean of hourly observations made here by myself, that any of these methods give means which, during every season of the year, are too high, and that the error in excess is greater, in proportion as the diurnal exceeds the nocturnal temperature. The last method, which is now in most general use, has probably been adopted in colder climates, as agreeing experimentally in its results with those derived from more frequently repeated observations during the same period; or possibly under the impression, that the 9 P.M. observation would give an approximation to the nocturnal mean, while the diurnal temperature would result as a mean from those at 7 A.M. and 2 P.M. In either case this method might be best adapted for many climates; but, as the relative value of the temperatures at times fixed for observation are not constant, but will vary in passing from one climate to another, there is nothing in the method to suit it for universal use. The fact is, it appears to me almost impossible to fix upon any method, except that of hourly observations, which will in every place afford even an approximation to the truth, or obtain harmonious results from elements the relations of which are so variable as those of the temperatures of particular hours. The method which I have adopted, and had in use about three years, I have found experimentally to give results, for this climate, almost in precise accordance with those derived from calculations based upon hourly observations. I have constantly kept daily observations at 6 and 7 A.M., 12 M., 2 P.M., sunset, and 9 P.M., so that I have been able to give my observation any of the common tabular forms, and have always obtained the general results according to each of the methods, and the results have verified my previous conclusions by varying from each other as follows: The mean annual temperature of this place (Jackson, La., U. S.), by my method, is 64.24°, Fahrenheit; as derived from the sunrise, 2 P.M., and sunset observations, 66.30°; and, during the same period, the mean obtained from the 7 A.M., 2 P.M., and 9 P.M. observations is 65.62°. In this way I have compared results by each method with each other, and with those obtained from hourly observations. I have found that the following method gives for this place results more accurate than any other I have been able to derive. The mean temperature of the diurnal portion of the twenty-four hours is derived as the mean of the highest and lowest temperatures of that portion; that is, of the sunrise observations and that of 2 P.M. The nocturnal mean will, in like manner, result as the mean of the highest and lowest temperatures of that portion; that is, of the sunset and sunrise observations. The average of these two means will give the mean for the twenty-four hours. By this method, the sunrise temperature being the lowest for the twenty-four hours, will belong to both the diurnal and nocturnal portions, and will enter twice into the calculations, while the other two observations will represent the maxima of the portions of the twenty-four hours to which they respectively belong.

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* Read before the Royal Scottish Society of Arts 8th May 1843.

If it is, as I suspect, then, that the results of observations and calculations are, particularly in hot countries, frequently too high, a little attention to the subjects above hinted at may shew that there is really no difference between the mean temperature of climates and of water derived from such depths as not to be affected by the change of seasons. In my tables the most exact agreement is shown between these temperatures at this place. The same appears to be the case in the island of Cuba, notwithstanding that we find the following statement in the table in which Professor Kupffer compares the annual temperature of places with that of the earth: "Havana, temperature of the earth, 74.30°; of the air, 78.12°." Now, Havana and Matanzas are in the same latitude, or nearly so, and there can be but little difference between the temperatures at the two places. Mr A. Mallary, in this Journal, vol. xxxi. p. 289, gives the annual temperature at Matanzas as 77.06°, Fahrenheit, which he derives as a mean of the following averages of observations: Sunrise 72.17°, 2 P.M. 81.41°, and sunset 77.61° Fahrenheit. Now, a mean of these observations would unquestionably give a mean too high by more than a degree, which would reduce the mean to about 76°. My friend Mr W. H. Potter has been kind enough to examine the temperature of wells, &c., for me, during a residence of a year in that island, particularly near Cardanus, in the same latitude and near Matanzas, and the observations frequently repeated through the year; and the temperature, apparently invariable was 76° Fahrenheit, thus affording a probability, at least, that there also the mean temperatute of the air and of the earth, at a certain depth, is the same.

What inferences might not be drawn by travellers in this country, particularly if they belonged to the anti-Huttonian school of geologists? I will not go out of my own neighbourhood. The temperature of water taken from strata whose average depth below the surface should be seventy or eighty feet, would be found to have at Baton Rouge a temperature of about 64.50° Fahrenheit. But we are informed by the tables of the Army Meteorological Register, that the annual mean for that place is 68.07°, making a difference of 3.57°. Now, we cannot mistrust the accuracy with which the observations were noted in this case, nor the correctness with which the calculations were made; but there can be no doubt that too high a temperature has been obtained for want of attention to things generally considered as of minor importance, and in consequence of the too general adoption of methods which have been found to give correct results in other climates.—*American Journal of Science and Arts*, vol. xliv., No. 1, p. 50.

Observations on the Temperature and Hygrometric State of the Atmosphere of the Island of Barbadoes. By ROBERT LAWSON, Esq., Assistant-Surgeon 47th Regiment.* Communicated by the Author.

Having in the course of service proceeded to the West Indies in the beginning of 1841, I gladly availed myself of the opportunity to make a series of observations on some branches of meteorology, which were more immediately within my reach, and which seemed likely to be of use in elucidating the action of climate on the constitution, believing that the results, limited although they be, have an important bearing on some of the atmospheric phenomena within the tropics, I am induced to lay them before the public. My attention was chiefly directed to the temperature of the atmosphere, the dew point, direction of the winds, and portion of the sky obscured by clouds. The observations were made three times daily, at 9 A.M., 3 P.M., and 9 P.M., with as much regularity as the nature of my duties would permit. They were commenced in May 1841, and continued until my return to England in February 1842.

Having no data wherewith to reduce the temperatures at the abovementioned hours to the mean of the day, I commenced, in June, a series of observations of the thermometer every hour between 5 A.M. and 10 P.M. (both inclusive), and which I was enabled, with but little interruption, to continue to the end of August. From this date, the pressure of duty was such, as to prevent my undertaking again the hourly observations even for a few days.

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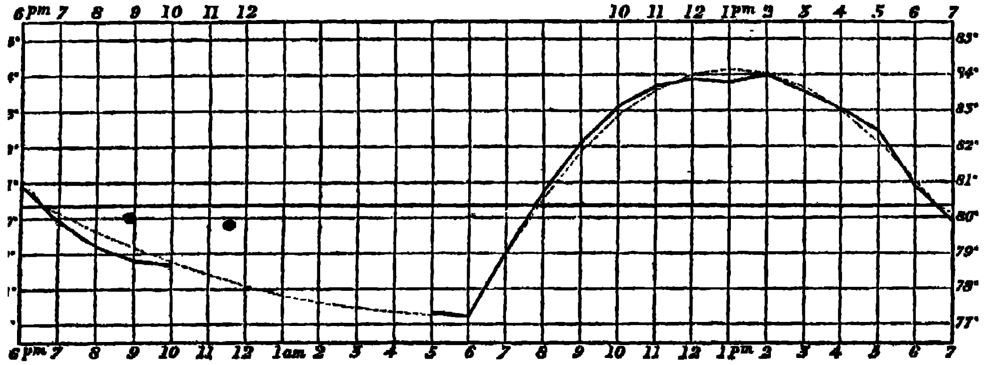
* Abstract of a Paper laid before the Royal Society of Edinburgh, March 7, 1843.

The errors of one of the thermometers employed in these observations, have been determined with a good deal of care, nearly in the manner recommended by Professor Forbes, in a paper embracing this subject, in the Philosophical Transactions, and those of the others were obtained by comparing them with this one. The errors of the register thermometers are, I believe, known every where to be about the tenth of a degree Fahrenheit, and those of the others certainly considerably within that quantity. All the observations have been corrected for the errors of the corresponding instruments.

DIURNAL CURVE OF TEMPERATURE AT BARBADOES DURING JUNE, JULY, AND AUGUST 1841.



The following observations were made at St Ann's, Barbadoes, which lies on the SW. side of the island, in lat. 13° 4′ N., and long. 59° 37′ W., and is about a mile and a half to the southward of Bridgetown. St Ann's is situated on a pretty extensive flat, resting on one side on the sea-shore. It is elevated about 30 feet above the level of the sea, and is perfectly open to it from the E.S.E. to the NW. To the northward and eastward the land rises gradually by a succession of flats to a moderate elevation, but does not seem to offer any obstacle to the free course of the trade winds. The distance from St Ann's to the windward side of the island, between E. and N.N.E., varies from 11 to 13 miles.

The thermometers were placed on the windward side of a building, inside jealousies, which were closed so as to prevent the action of reflected light or heat, though a current of air at all times passed freely through. Outside the jealousies was a wide verandah, perfectly open to windward, which effectually screened the instruments from the direct action of the sun. The room was occupied as a sleeping apartment, but the jealousies to leeward were constantly open, and arrangements were made to admit of the thermometers being exposed to a constant current of air from without, while the communication of heat from within was as much diminished as possible. There was never a fire in the room, and lights were employed in it only a few minutes at a time, to read off the temperatures; that notwithstanding these precautions, the temperatures near the hour of minimum were somewhat affected by the slight excess of heat in the room and surrounding materials, above that of the air, I have no doubt; but am inclined to think that it must be very slight, and will not materially affect the deductions to be drawn from the whole body of observations.

The following observations were made at mean time, as shewn by a good watch, the errors and rate of which were found by altitudes of the sun once a week. Every observation was made by myself, and I think that nowhere in the hourly observations can the difference, on the mean of a month, between the real time of observation and corresponding hour, exceed a minute either way.

Table I., shewing the means of Temperature for each hour in the months of June, July, and August 1841, at Barbadoes. Also the whole number of Observations at each hour, and the corresponding mean Temperatures for the three months, together with a series of numbers calculated on the supposition that the diurnal changes of Temperature may be represented by parabolic curves.

Hour.	Hourly Observations.			In three Months.			Difference between observed and calculated.
	June.	July.	August.	No. of Observations at each hour.	Mean Temp. at each hour.	Temp. calculated.	
A.M. 5.	77°.30	77°.30	77°.32	77	77°.31	77°.26	—°.05
6.	77.26	77.25	77.21	85	77.24	77.24	+ .00
7.	79.16	78.76	79.32	80	79.08	79.07	— .01
8.	80.50	80 39	81.37	84	80.75	80.62	— .13
9.	82.03	81.67	82.80	89	82.17	81.89	— .28
10.	83.10	82.60	83.81	85	83.17	82.88	— .29
11.	84.05	83.02	83.95	73	83.07	83.59	— ·08
12.	84.15	83.36	84.20	71	83.90	84.01	+ .11
P.M. 1.	83.93	83.36	84.15	86	83.81	84.15	+ .34
2.	84.30	83.38	84.25	83	83.98	84.03	+ .05
3.	83.60	83.31	83.77	87	83.56	83.66	+ .10
4.	83.17	82.72	83.38	84	83.09	83.05	— .04
5.	82.29	82.05	83.14	81	82.49	82.20	— .29
6.	80.82	80.55	81.45	75	80.94	81.10	+ .16
7.	80.24	79.37	81.15	86	79.92	80.10	+ .21
8.	79.46	79.28	79.02	49	79.25	79.63	+ .38
9.	79.08	78.68	78.81	82	78.86	79.18	+ .32
10.	78.86	78.66	78.56	69	78.60	78.77	+ .08
11.	...	...	...	Calculated.	78.41	78.41	
12.	...	...	...		78.10	78.10	
A.M. 1.	...	...	...		77.84	77.84	
2.	...	...	...		77.62	77.62	
3.	...	...	...		77.46	77.46	
4.	...	...	...		77.34	77.34	
Means.	81.294	80.873	81.481	1426	80.364	80.385	

As no observations were made from 11 P.M. to 4 A.M., it was necessary that this gap should be filled up, before the mean temperature could be found. Sir David Brewster had long ago shewn, in the case of the thermometrical observations at Leith Fort, that the curve of daily temperature might be represented, with considerable accuracy, by parabolic arcs. On projecting the above observations, their resemblance to parabolic curves was sufficiently obvious, though differing in their arrangement considerably from those at Leith. In the present case, the 10 P.M. observation gives a temperature less by 0°.08 only than that obtained by calculation, and the 5 A.M. observation one greater by 0°.05; consequently, the actual curve of temperature must have crossed that obtained by calculation, between 10 P.M. and 5 A.M.; and it is not likely, that, had the observations been continued at the intermediate hours, the resulting temperatures would have differed materially from those afforded by calculation; and, as any difference that may exist between the latter and former will affect the mean temperature, derived from the whole observations, by only one-fourth of its amount, I have not hesitated to insert them in the above table, in place of actual observations, in order to derive from the whole the mean temperature for the three months.

The mean temperature of June, July, and August 1841, in Barbadoes, is thus found to be 80°.364 Fah.; and the diurnal curve of temperature crosses it at 7 h. 46 m. A.M., and again at 6 h. 34 m. P.M. The diurnal changes of temperature will be readily comprehended by inspecting the accompanying drawing, in which the broad horizontal line indicates the mean temperature, 80°.364, while the continuous curved line represents the curve, as derived from observation, and the dotted line, the results obtained by calculation. The actual curve of temperature presents a remarkable depression between mid-day and 2 P.M. This is not accidental, but is owing to the air, which is in immediate contact with the ground, then becoming much heated, which causes it to ascend, when its place is supplied by colder air from above. About mid-day, especially when a fresh trade wind is blowing, the thermometer is in a constant state of oscillation from this cause, and it is not unfrequently seen to alter even a whole degree in 15 or 20 seconds.

As the observations on each side of 1 P.M. seem to indicate that as very nearly the hour of maximum temperature (supposing the depression in the curve above noticed had not existed) of the day, I have in the calculation assumed it to be so, and have farther supposed the temperature then to be such as would best represent the afternoon portion of the curve according to observation, that being the part of the day when the danger of error from radiation was least, and the breeze generally strongest. The hour which gives the lowest temperature is 6 A.M., though the minimum of the 24 hours, during July and August, actually occurred about 5h. 45 m. A.M., nearly the time of sunrise. By inspecting the drawing, it is obvious that those portions of the diurnal curve, extending from 1 P.M. backwards to the hour of minimum, and from 1 P.M. to the point of mean temperature in the evening, and that extending from the hour of minimum backwards to the same point, are closely represented by the corresponding parabolic arcs; and it is highly probable, that, had these observations been continued for twelve months, the agreement would have been still closer.

The mean of the 18 hourly observations for the three months is 81°.216, which is an excess of the actual mean temperature of the period, 80°.364, by 0°.852. Subtracting this quantity from the mean of the observations for each month, the result must be very nearly the mean temperature of the corresponding month. By this process, 80°.442, 80°.021, and 80°.629, are obtained as the mean temperatures of June, July, and August respectively. The mean of these observations at 9 A.M., 3 P.M., and 9 P.M., for the three months, is in excess of the mean temperature of the period by 1°.166; and, if the mean of the observations at these hours, for each, be reduced by this quantity, 80°.404, 80°.054, and 80°.627, are obtained as the mean temperatures for the respective months—temperatures differing so little from those obtained from 18 hourly observations, that, for all practical purposes, they may be safely substituted.

The observations, of which Table II. is an abstract, were commenced on the 11th May 1841, and were continued till the beginning of February 1842. Subsequently to October, I was unable to make the observations exactly at the hour; but the deviations having been noted, a correction for the mean amount, derived from the hourly observations in June, &c., has been applied to the results in the table. The mean deviation in a month nowhere exceeded ten minutes, and no single observation was made more than thirty minutes from the corresponding hour. Notwithstanding the latitude thus taken, the observations at 3 P.M. and 9 P.M., in the last two months, were frequently omitted, from unavoidable circumstances; but, as in neither case did the omission exceed one-third of the whole number that ought to have been made at 9 P.M., nor one fourth of those that ought to have been made at 3 P.M., the means given in the table, when the great regularity of the atmospheric phenomena in that climate is considered, will be found not undeserving of confidence. From October onwards, the minimum is probably about 0°.2 Fah. too high.

The dew point was calculated by Dr Apjohn's Tables, from the indications of a wet bulb thermometer. The table of elasticity of vapour employed was that given in the appendix to the report of the Committee of the Royal Society on Physics and Meteorology. In the reduction the height of the barometer was uniformly assumed to be 30 inches.

Table II., Monthly Abstract of Daily Meteorological Register kept at St Ann's, Barbadoes, from 11th May 1841 to 31st January 1842.

MONTH.	TEMPERATURE.						DEW POINT.				PORTION OF SKY CLOUDED AT				No. of days on which rain fell.	Prevalent Winds.
	9 A.M.	3 P.M.	9 P.M.	Mean Max.	Mean Min.	Mean Temp.	9 A.M.	3 P.M.	9 P.M.	Mean.	9 A.M.	3 P.M.	9 P.M.	Mean.		
May	81.23	82.75	78.82	83.86	77.04	79.769	70.08	69.85	70.52	70.15	0.53	0.50	0.41	0.48	13	E. ESE. SE.
June	82.03	83.60	79.08	85.23	76.74	80.404	71.17	70.71	71.53	71.14	0.50	0.50	0.25	0.42	16	E. ESE. ENE.
July	81.67	83.31	78.68	84.72	77.02	80.054	71.81	70.60	72.17	71.53	0.59	0.53	0.38	0.50	19	ENE. E.
Augt.	82.80	83.77	78.81	85.63	77.05	80.627	71.58	70.61	72.08	71.42	0.44	0.44	0.31	0.40	16	ENE. E. ESE.
Sept.	81.98	82.13	78.12	84.09	77.39	79.577	73.93	73.78	73.62	73.78	0.66	0.66	0.31	0.54	15	ENE. SE. E.
Oct.	81.81	82.31	78.53	84.77	76.62	79.717	72.93	72.67	72.97	72.86	0.50	0.56	0.34	0.47	11	ENE. ESE. E.
Nov.	82.13	82.83	78.12	84.86	76.54	79.861	71.45	71.67	71.64	71.59	0.38	0.47	0.22	0.36	10	ENE. E. SE.
Dec.	78.37	80.26	75.24	81.71	73.65	76.791	68.94	68.23	68.61	68.59	0.50	0.47	0.34	0.44	17	ENE.
Jan.	77.98	79.30	74.55	81.20	72.97	76.[illegible]11	67.86	66.92	67.93	67.57	0.42	0.37	0.18	0.32	15	ENE. E.
Means.	81°.11	82°.25	77°.77	84°.01	76°.11	79°.212	71°.08	70°.56	71°.23	70°.96	0.50	0.50	0.30	0.44		

The means of temperature for 9 A. M., 3 P. M., and 9 P. M., in the above table, for nine months, indicate a curve very closely resembling that derived from the observations in June, July, and August. The differences between the temperatures at these hours, for the whole period, are rather less than between those for June, &c.; but as the mean diurnal variation, as shewn by the register thermometers, is less in the former than in the latter case, in almost exactly the same proportion, the diminution of the differences between the temperatures at the respective hours, the consequence of a somewhat flatter curve, was to be expected. The 9 A. M. observation, for the whole period, approaches that of 3 P. M. more than, according to the hourly observations, it should do; but when it is recollected, that in the months given in the table, almost the whole of those in which the equation of time is additive to mean time, are included, and in which, consequently, the sun's hour angle is less, and this heating power in the forenoon greater than indicated by mean time, while February and March, during which the equation of time is subtractive, and reaches its greatest amount, are omitted, there is good reason to suppose, that, had the observations been continued for twelve months, the results obtained would have presented a perfectly satisfactory agreement with those afforded by the hourly observations. These results will, in the absence of more extensive materials, afford data for arriving at a very close approximation to the annual mean temperature of Barbadoes, from any series of observations, and they will, in all probability, be equally applicable to all the lesser West India Islands, and to the flat portions of the adjacent coast of South America, where the free course of the trade winds is not interrupted by mountains.

The dew point in the table is calculated from the indications of a wet bulb thermometer; to show how far these could be depended on, I frequently determined the dew point directly, in September and October, and again in December and January, by cooling a small quantity of water in a piece of common test tube, by powdered nitre, the temperature of the mixture being obtained, at the moment of deposition of dew on the surface of the tube, by a delicate thermometer, with which the mixture was constantly stirred until the deposition took place. This method, with a little management, is capable of great precision.

During September and October 31, observations of the dew point were obtained at 9 A. M., and 28th at 3 P. M. These, and the corresponding results from the wet bulb thermometer, at the same hours, and on the same days, are as under—

	9 A. M.	3 P. M.
Dew point calculated, ...	73°.44	73°.63
,, observed, ...	73 .04	73 .24
Actual calculated in excess of the dew point,	0°.40	0°.39

In December and January, only fourteen direct determinations were obtained at 3 P. M. These being treated in the same manner, give the calculated dew point 66°.13, and the observed 68°.56,—the former being in excess by 0°.27. These experiments made, respectively, when the moisture and stillness of the atmosphere were greatest, and when they were about least, will give a pretty accurate idea of how far the indications of the wet bulb thermometer may be depended on in the West Indies.

A similar depression occurs in the dew point at 3 P. M., in most of the months, to that already noticed in the temperature of the air about midday, and which was obviously attributable to the same cause. The variations of the dew point from day to day, though sufficiently marked, were never very great; it was always higher with a south-easterly wind than with the regular trade, unless immediately after rain; and when the moon's declination was north, the air being then generally stiller than when it was to the south of the equator, the dew point was usually considerably above the average for the corresponding month.

The cloudiness of the sky varied considerably in different months, and scarcely less so at various periods of the same month; its effect on the temperature, as was to be expected, and, indeed, as is obvious from the table, was very considerable. From the impossibility of procuring an eligible site for a rain-gauge, which, at the same time, might be free from interruption, I was unable to form any idea of the quantity that fell. The number of days on which it occurred are given in table I.; and, though the year was accounted unusually dry, it is found that showers, frequently heavy, fell, on the average, every second day. Heavy and continuous rain always came from the southward of east; but up to October, when the moon's declination was northerly, very heavy showers from the E.N.E. or NE. were common.

The prevailing winds are given in the table, in the order of their prevalence, during each month. The regular trade wind from the E.N.E. predominates; and the others, with very few exceptions, are but varieties of it. The generally received explanation of the cause of the trade winds is accurate in the main; but there are several most important modifications, which seem to be chiefly owing to the differences in the moon's declination and time of transit, and which, if fairly examined into, bid fair to throw much light on the origin of, if not actually to account for, the phenomena of the West Indian hurricane.

Quantity of Salt in Sea-Water, as determined by the examination of specimens collected in a Voyage from Southampton to New York. By Dr DAUBENY, F.R.S., &c. &c.

LOCALITY.	LATITUDE.	LONGITUDE.	Sp. gr. of Water.	Proportion of solid matter in 500 grs. obtained by evaporation in a water-bath.
Off Southampton,	50.54	1.24	1027.00	19.40
Water taken up during a voyage between Portsmouth and New York in 1837, . . .	49.38	2.0	10267.20	20.4
	49 10	4.0	10269.08	...
	49 28	6 36	10269.99	20.95
	50 5	9.19	10269.99	
	50.0	12.0	10269.99	
	49.34	12.7	10270.90	. .
	47.27	13.35	10271.81	20.9
	48.50	15.30	10271.81	
	48.40	17.40	10271.81	
	46.45	17 34	10272.72	20.85
	44.40	20 15	10272.72	
	43.41	21.42	10272.72	
	43.[illegible]	24 18	10275.45	21.00
	4[illegible]	28 38	10275.45	
	43.30	[illegible]	10274.54	...
	44 45	33 [illegible]	10272.72	...
	45 12	34.52	10273.63	...
	45.36	37.3	10271.81	...
	45 40	40.14	10269.08	. .
	42.49	45 45	10272.72	...
	41.10	48 23	10254.52	...
	41.30	50.18	10249 06	18.9
	42.30	52 10	10249.06	
	44.0	53.51	10249.97	...
	42.52	57.18	10248 15	..
	42 52	57.58	10249.06	...
	42.35	62.00	10254.28	18.7
	41 0	65.43	10254.28	
	40 40	67.21	10256.34	...
	39 50	69.27	10249.06	...
	39.27	71.13	10265.44	19.2
Off Sandy Hook,	...	...	10229.04	...
Drawn from a depth of 80 fathoms,	41.10	48.23	10265.44	...
Of 100 fathoms,	39.54	67.34	10273.63	21.0
Surface-water nearest to above,	...	...	10254.28	18.7
				2.3 grs. diff.

The above results indicate a pretty uniform rate of increase in the saltness of the sea, as we recede from either coast.

Examination of Specimens of Sea-Water, collected from different localities. By Dr DAUBENY, F.R.S., &c. &c.

LOCALITY.	LATITUDE.	LONGITUDE.	Sp. gr. of Water.	Proportion of solid matter in 500 grs. obtained by evaporation in a water-bath.
Between Charleston and Havanna,	28.16	80.8	10258.16	...
	27.30	...	10277.27	...
	24.40	...	10273.63	20.90
	23.28	...	10273.63	
	23.15	...	10276.36	...
Gulf of Mexico between Havanna and N. Orleans,	24.23	84.30	10276.36	...
	26.33	86.47	10275.45	...
	28.20	89.00	10278.18	21.1
Indian Ocean,	Equator.	84.0 E.	1026.00	19.00
Ditto,	Ditto.	8.16	1025.90	19.23
Ditto,	Ditto.	Do. depth 625 ft.	1027.47	20.88
Atlantic Ocean,	Ditto.	19.30 W.	1026.70	19.10
Bay of Naples,	4 0.50	14.15	1030.00	22.30
Marseilles,	43.17	5 .22	1031.00	23.10

Dr Daubeny subjoins those obtained some years ago by Dr Marcet, as reported in the Philosophical Transactions, viz.:—

LOCALITY.	LATITUDE.	LONGITUDE.	Sp. gr. of Water.	Proportion of solid matter in 500 grs. obtained by evaporation in a water-bath.
Atlantic Ocean,	Equator.	23.0 W.	1027.85	19.6
South Atlantic,	21.0	0.0	1028.19	20.6
North Atlantic,	25.30	32.30	1028.86	21.3

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Historical Notices in regard to the distinction of Nerves and Nervous Filaments into Motive and Sensitive; and in regard to the peculiarity of function, and absolute isolation, of the ultimate Nervous Filaments. By Sir WILLIAM HAMILTON, Bart.

TO PROFESSOR JAMESON.

MY DEAR SIR,—As I see that public interest is again turned towards Sir CHARLES BELL'S great discovery regarding the functions of the nervous system, I send you a note, belonging to a forthcoming philosophical work, and containing a summary of what I have noticed touching the history of physiological discovery in this relation. If you think it of any value, you are welcome to make whatever use of it for your Journal you may think fit.

I remain, my Dear Sir, with great regard, ever truly yours,

W. HAMILTON.

EDINBURGH, *May* 31. 1843.

The important discovery of Sir Charles Bell, that the spinal nerves are the organs of motion through their anterior roots, of sensation through their posterior; and the recognition by recent physiologists, that each ultimate nervous filament is distinct in function, and runs isolated from its origin to its termination;—these are only the last of a long series of previous observations to the same effect,—observations, in regard to which (as may be inferred from the recent discussions touching the history of these results) the medical world is, in a great measure, uninformed. At the same time, as these are the physiological facts with which psychology is principally interested; as a contribution towards this doctrine and its history, I shall throw together a few notices, which have, for the most part, fallen in my way when engaged in researches for a different purpose.

1. The cases of paralysis without narcosis (stupor), and of narcosis without paralysis—for the ancient propriety of these terms ought to be observed—that is, the cases in which either motion or sensibility, exclusively, is lost, were too remarkable not to attract attention even from the earliest periods; and at the same time, too peremptory not to necessitate the conclusion, that the several phænomena are, either the functions of different organs, or, if of the same, at least regulated by different conditions. Between these alternatives all opinions on the subject are divided; and the former was the first, as it has been the last, to be adopted.

No sooner had the nervous system been recognised as the ultimate organ of the animal and vital functions, and the intracranial medulla or encephalos (*encephalon* is a modern misnomer) ascertained to be its centre, than ERASISTRATUS proceeded to appropriate to different parts of that organism the functions which, along with Herophilus, he had distinguished, of sensibility and voluntary motion. He placed the source of the former in the meninges or membranes, of the latter in the substance, of the encephalos in general, that is, of the Brain-proper and After-brain or Cerebellum. And while the nerves were, mediately or immediately, the prolongations of these, he viewed the nervous membranes as the vehicle of sensation, the nervous substance as the vehicle of motion. (Rufus Ephesius, L. i. c. 22; L. ii. cc. 2, 17.) This theory, which is remarkable, if for nothing else, as manifesting the tendency from an early period to refer the phenomena of motion and sensation to distinct parts of the nervous organism, has not obtained the attention which it even intrinsically merits. In modern times, indeed, the same opinion has been hazarded, even to my fortuitous knowledge, at least thrice. Firstly by Fernelius (1550, Physiologia, L. v. cc. 10, 14;) secondly by Rosetti (1722, Raccolta d'Opuscoli, &c., t. v. p. 272 sq.;) thirdly by Le Cat (1740, Traité des Sensations, Œuv. Phys. t. i. p. 124, and Diss. sur la Sensibilité des Meninges, § i.) By each of these the hypothesis is advanced as original. In the two last this is not to be marvelled at; but it is surprising how the opinion of Erasistratus could have escaped the erudition of the first. I may observe, that Erasistratus also anticipated many recent physiologists in the doctrine, that the intelligence of man, and of animals in general, is always in proportion to the depth and number of the cerebral convolutions, that is, in the ratio of the extent of cerebral surface, not of cerebral mass.

The second alternative was adopted by GALEN, who, while he refutes, apparently misrepresents, the doctrine of Erasistratus; for Erasistratus did not, if we may credit Rufus, an older authority than Galen, derive the nerves from the membranes of the encephalos, to the exclusion of its substance; or if Galen be herein correct, this is perhaps the early doctrine which Erasistratus is by him said in his maturer years to have abandoned;—a doctrine, however, which, under modifications, has in modern times found supporters in Rondeletius and others. (Laurentii Hist. Anat. L. iv. qu. 13.)—Recognising, what has always indeed been done, the contrast of the two phenomena of sensibility and motion, Galen did not, however, regard them as necessarily the products of distinct parts of the nervous system, although, de facto, different parts of that system were often subservient to their manifestation. As to the problem—Do the nerves perform their double function by the conveyance of a corporeal fluid, or through the irradiation of an immaterial power?—Galen seems to vacillate; for texts may be adduced in favour of each alternative. He is not always consistent in the shares which he assigns to the heart and to the brain, in the elaboration of the animal spirits; nor is he even uniform in maintaining a discrimination of origin, between the animal spirits and the vital. Degrading the membranes to mere envelopments, he limits every peculiar function of the nervous organism to the enveloped substance of the brain, the after-brain, the spinal chord and nerves. But as the animal faculty is one, and its proximate vehicle, the animal spirits, is homogeneous, so the nervous or cerebral substance which conducts these spirits is in its own nature uniform, and indifferently competent to either function; it being dependent upon *two accidental circumstances*, whether this substance conduce to motion, to sensation, or to motion and sensation together.

The first circumstance is the degree of *hardness* or *softness*; a nerve being adapted to motion, or to sensation, in proportion as it possesses the former quality or the latter. Nerves extremely soft are exclusively competent to sensation. Nerves extremely hard are pre-eminently, but not exclusively, adapted to motion; for no nerve is wholly destitute of the feeling of touch. The soft nerves, short and straight in their course, arise from the anterior portion of the encephalos (the Brain proper;) the hard, more devious in direction, spring from the posterior portion of the brain where it joins the spinal chord (Medulla oblongata?) the spinal chord being a continuation of the After-brain, from which no nerve immediately arises; the hardest originate from the spinal chord itself, more especially towards its inferior extremity. A nerve soft in its origin, and therefore fitted only for sense, may, however, harden in its progress, and by this change become suitable for motion.

The second circumstance is the *part* to which a nerve is sent; the nerve being sensitive or motive as it terminates in an organ of sense, or in an organ of motion—a muscle; every part being recipient only of the virtue appropriate to its special function.

This theory of Galen is inadequate to the phænomena. For, though loss of motion without loss of sense may thus be accounted for, on the supposition that the innervating force is reduced so low as not to radiate the stronger influence required for movement, and yet to radiate the feebler influence required for feeling, still this leaves the counter case (of which, though less frequently occurring, Galen has himself recorded some illustrious examples) not only unexplained, but even renders it inexplicable. In this theory Galen is, likewise, not always consistent with himself. The distinction of hard and soft, as corresponding with the distinction of motory and sensitive nerves, though true in general, is, on his own admission, not absolutely through-going. (I must observe, however, that, among other recent anatomists, this is maintained by Albinus, Malacarne, and Reil.) And to say nothing of other vacillations, Galen, who in one sentence, in consistency with his distinction of cerebral and (mediately) cerebellar nerves, is forced to accord exclusively to those of the spine the function of motion; in another finds himself compelled, in submission to the notorious fact, to extend to these nerves the function of sensation likewise. But if Galen's theory be inadequate to their solution, it never leads him to overlook, to dissemble, or to distort, the phænomena themselves; and with these no one was ever more familiarly acquainted. So marvellous, indeed, is his minute knowledge of the distribution and functions of the several nerves, that it is hardly too much to assert, that, with the exception of a few minor particulars, his pathological anatomy of the nervous system is practically on a level with the pathological anatomy of the present day. (De Usu Partium, i. 7, v. 9, 7, 14, viii. 3, 6, 10, 12, ix. 1, xii. 10, 11, 15, xiii. 8, xvi. 1, 3, 5, xvii. 2, 3.—De Causis Sympt. i. 5.—De Motu Musc. 1. 13.—De Anat. Adm. vii. 8.—Ars Parva, 10, 11.—De Locis Aff. i, 6, 7, 12, iii. 6, 12.—De Diss. Nerv. 1. —De Plac. Hipp. et Plat. ii. 12, vii. 3, 4, 5, 8.)

2. The next step was not made until the middle of the fourteenth century, subsequent to Galen's death; when Rondeletius (c. 1550,) reasoning from the phænomena of paralysis and stupor, enounced it as an observation never previously made, that " All nerves, from their origin in the brain, are, even in the spinal marrow itself, isolated from each other. The cause of paralysis is therefore not so much to be sought for in the spinal marrow as in the encephalic heads of the nerves; Galen himself having, indeed, remarked, that paralysis always supervenes when the origin of the nerve is obstructed or diseased." (Curandi Methodus c. 32.)

This observation did not secure the attention which it deserved; and some thirty years later (1595,) another French physiologist, another celebrated professor in the same university with Rondelet, I mean Laurentius of Montpellier, advanced this very doctrine of his predecessor, as "a new and hitherto unheard-of observation." This anatomist has, however, the merit of first attempting a sensible demonstration of the fact, by resolving, under water, the spinal cord into its constituent filaments. " This new and admirable observation," he says, " explains one of the obscurest problems of nature; why it is that from a lesion, say of the cervical medulla, the motion of the thigh may be lost, while the motions of the arms and thorax shall remain entire." In the second edition of his Anatomy, Dulaurens would seem, however, less confident, not only of the absolute originality, but of the absolute accuracy of the observation. Nor does he rise above the Galenic doctrine, that sensibility and motion may be transmitted by the same fibre. In fact, rejecting the discrimination of hard and soft nerves, he abolishes even the accidental distinction which had been recognised by Galen. (Compare Hist. Anat., later editions, iv. c. 18, qq. 9, 10, 11; x. c. 12, with the relative places in the first.)

3. The third step was accomplished by Varollius, (1572), who shewed Galen to be mistaken in holding that the spinal chord is a continuation of the After-brain alone. He demonstrated against all previous anatomists, that this chord is made up of four columns, severally arising from four encephalic roots; two roots or trunks from the Brain-proper being prolonged into its anterior, and two from the After-brain into its posterior columns. (Anatomia, L. iii; De Nervis Opticis Epistolæ.)

At the same time, the fact was signalized by other contemporary anatomists (as Coiter, 1572, Laurentius, 1595), that the spinal nerves arise by double roots; one set of filaments emerging from the anterior, another from the posterior, portion of the chord. It was in general noticed, too (as by Coiter, and C. Bauhinus, 1590), that these filaments, on issuing from the chord, passed into a knot or ganglion; but, strange to say, it was reserved for the second Monro (1783), to record the special observation, that this ganglion is limited to the fibres of the posterior root alone.

Such was the state of anatomical knowledge touching this point, at the close of the sixteenth century; and it may now seem marvellous, that, aware of the independence of the motory and sensitive functions,—aware that of these functions the cerebral nerves were, in general, limited to one, while the spinal nerves were competent to both,—aware that the spinal nerves, the nerves of double function, emerged by double roots and terminated in a twofold distribution,—and, finally, aware that each nervous filament ran distinct from its peripheral extremity through the spinal chord to its central origin;—aware, I say, of all these correlative facts, it may now seem marvellous that anatomists should have stopped short, should not have attempted to lay fact and fact together, should not have surmised that in the spinal nerves difference of root is correspondent with difference of function, should not have instituted experiments, and anticipated by two centuries the most remarkable physiological discovery of the present day. But our wonder will be enhanced, in finding the most illustrious of the more modern schools of medicine teaching the same doctrine in greater detail, and yet never proposing to itself the question—May not the double roots correspond with the double function of the spinal nerves? But so has it been with all the most momentous discoveries. When Harvey proclaimed the circulation of the blood, he only proclaimed a doctrine necessitated by the discovery of the venous valves; and the Newtonian theory of the heavens was but a final generalization prepared by foregone observations, and even already partially enounced.

The school I refer to is that of Leyden—the school of Boerhaave and his disciples.—Boerhaave held with Willis that the brain-proper is the organ of animality; a distinct part thereof being destined to each of its two functions, sense and voluntary motion;—that the After-brain is the organ of vitality, or the involuntary motions;—and that the two encephalic organs are prolonged, the former into the anterior, the latter into the posterior, columns of the spinal chord. In his doctrine, all nerves are composite, being made up of fibrils of a tenuity, not only beyond our means of observation, but almost beyond our capacity of imagination. Some nerves are homogeneous, their constituent filaments being either for a certain kind of motion alone, or for a certain kind of sensation alone; others are heterogeneous, their constituent fibrils being some for motion, some for sensation;—and of this latter class are the nerves which issue from the spine. On Boerhaave's doctrine, however, the spinal nerves, in so far as they arise from the anterior column, are nerves both of sensation and voluntary motion—of animality; in so far as they arise from the posterior column, are nerves of involuntary motion—of vitality. A homogeneous nerve does not, as a totality, perform a single office; for every elementary fibril of which it is composed runs from first to last isolated from every other, and has its separate sphere of exercise. As many distinct spheres of sensation and motion, so many distinct nervous origins and terminations; and as many different points of local termination in the body, so many different points of local origin in the brain. The Sensorium Commune, the centre of sensation and motion, is not therefore an indivisible point, not even an undivided place; it is, on the contrary, the aggregate of as many places (and millions of millions there may be) as there are encephalic origins of nervous fibrils. No nerve, therefore, in propriety of speech, gives off a branch; their sheaths of dura mater alone are ramified; and there is no intercourse, no sympathy between the elementary fibrils, except through the sensorium commune. That the nerves are made up of fibrils is shewn, though inadequately, by various anatomical processes; and that these fibrils are destined for distinct and often different purposes, is manifested by the phænomena of disjoined paralysis and stupor. (De Morbis Nervorum Prælectiones, by Van Eems, pp. 261, 490–497, 696, 713–717. Compare *Kaau Boerhaave,* Impetum Faciens, § 197–200.)

The developed doctrine of Boerhaave on this point is to be sought for, neither in his Aphorisms, nor in his Institutions and his Prelections on the Institutions—the more prominent works to which his illustrious disciples, HALLER and VAN SWIETEN, appended respectively a commentary.—Of these, the *latter* adopts, but does not advance, the doctrine of his master. (Ad Aph. 701, 711, 774, 1057, 1060.)—The *former,* who, in his subsequent writings, silently abandoned the opinion, that sensation and motion are conveyed by different nervous fibrils, in two unnoticed passages of his annotations on Boerhaave (1740), propounds it as a not improbable conjecture—that a total nerve may contain within its sheath a complement of motory and of sensitive tubules, distinct in their origin, transit, and distribution, but which at their peripheral extremity communicate; the latter, like veins, carrying the spirits back to the brain, which the former had, like arteries, carried out. (Ad Boerh. Instit. § 288, n. 2, § 293, n. 2.)

The doctrine of the school of Leyden, on this point, was, however, still more articulately evolved by the younger (Bernard Siegfried) ALBINUS; not in any of his published works, but in the prelections he delivered for many years, in that university, on Physiology. From a copy in my possession of his dictata in this course, very fully taken, after the middle of the century, by Dr William Grant (of Rothiemurchus), subsequently a distinguished medical author and practical physician in London, compared with another very accurate copy of these dictata, taken by an anonymous writer, in the year 1741; I am enabled to present the following general abstract of the doctrine taught by this celebrated anatomist, though obliged to retrench both the special cases, and the reasoning in detail by which it is illustrated and confirmed.

The nerves have a triple destination as they minister (1.) to voluntary motion, (2.) to sensation, (3.) to the vital energies—secretion, digestion, &c. Albinus seems to acquiesce in the doctrine, that the Brain-proper is the ultimate organ of the first and second function, the After-brain, of the third.

Nerves, again, are of two kinds. They are either such in which each ultimate fibril remains isolated in function from centre to periphery (the cerebro-spinal nerves); or such in which these are mutually confluent (the sympathetic or ganglionic nerves.)

To speak only of the cerebro-spinal nerves, and of these only in relation to the functions of motion and sensation;—they are to be distinguished into three classes according as destined, (1.) to sense, (2.) to motion, (3.) to both motion and sensation. Examples—of the first class are the olfactory, the optic, the auditory, of which last he considers the portio mollis and the portio duris to be, in propriety, distinct nerves;—of the second class, are the large portion of those passing to muscles, as the fourth and sixth pairs; of the third class, are the three lingual nerves, especially the ninth pair, fibrils of which he had frequently traced, partly to the muscles, partly to the gustatory papillæ of the tongue, and the subcutaneous nerves, which are seen to give off branches, first to the muscles, and thereafter to the tactile papillæ of the skin. The nervous fibres which minister to motion are distinct in origin, in transit, in termination, from those which minister to sensation. This is manifest in the case of those nerves which run from their origin in separate sheaths, either to an organ of sense (as the olfactory and optic), or to an organ of motion (as the fourth and sixth pairs, which go to the muscles of the eye); but it is equally, though not so obtrusively, true, in the case where a nerve gives off branches partly to muscles, partly to the cutaneous papillæ. In this latter case, the nervous fibrils or fistulæ are, from their origin in the medulla oblongata to their final termination in the skin, perfectly distinct. The medulla oblongata is a continuation of the encephalos, made up of two columns from the Brain-proper, and of two columns from the After-brain. Immediately or mediately, it is the origin, as it is the organ, of all the nerves. And in both respects it is double: for, one part, the organ of sense, affords an origin to the sensitive fibrils, whilst another, the organ of motion, does the same by the motory. In their progress, indeed, after passing out, the several fibrils, whether homogeneous or not, are so conjoined by the investing membranes as to exhibit the appearance of a single nerve; but when they approach their destination they separate, those for motion ramifying through the muscles, those for sensation going to the cutaneous papillæ or other organs of sense. Examples of this are afforded—in the ninth pair, the fibres of which (against more modern anatomists) he holds to arise by a double origin in the medulla, and which, after running in the same sheath, separate according to their different functions and destinations;—and in the seventh pair, the hard and soft portions of which are respectively for motion and for sensation, though these portions, he elsewhere maintains, ought rather to be considered as two distinct nerves than as the twofold constituents of one.

The proof of this is of various kinds.—In the *first* place, it is a theory forced upon us by the phenomena; for only on this supposition can we account for the following facts:—(1.) That we have distinct sensations transmitted to the brain from different parts of the same sensitive organ (as the tongue) through which the same total nerve is diffused. (2.) That we can send out from the brain a motive influence to one, nay, sometimes to a part of one, muscle out of a plurality, among which the same total nerve (*e. g.* the ischiatic) is distributed. (3.) That sometimes a part is either, on the one hand, paralysed, without any loss of sensibility, or, on the other, stupified, without a diminution of its mobility.

In the *second* place, we can demonstrate the doctrine, proceeding both from centre to periphery, and from periphery to centre. Though ultimately dividing into filaments beyond our means of observation, we can still go far in following out a nerve both in its general ramifications, and in the special distribution of its filaments. for motion to the muscles and for sensation to the skin, &c.; and how far soever we are able to carry our investigation, we always find the least fibrils into which we succeed in analyzing a nerve, equally distinct and continuous as the chord of which they were constituent. And again, in following back the filaments of motion from the muscles, the filaments of sensation from the skin, we find them ever collected into larger and larger bundles within the same sheath, but never losing their individuality, never fused together to form the substance of a larger chord. The nerves are thus not analogous to arteries, which rise from a common trunk, convey a common fluid, divide into branches all similar in action to each other and to the primary trunk. For every larger nerve is only a complement of smaller nerves, and every smallest nerve only a fasciculus of nervous fibrils; and these not only numerically different, but often differing from each other in the character of their functions.

In the *third* place, that in the nerves for both motion and sensation are enveloped distinct nerves or fibrils for these several functions—this is an inference supported by the analogy of those nerves which are motive or sensitive exclusively. And in regard to these latter, it becomes impossible, in some cases, to conceive why a plurality of nerves should have been found necessary, as in the case of the two portions of the seventh pair, in reality distinct nerves, if we admit the supposition that each nerve, each nervous fibril, is competent to the double office.

In the *fourth* place, the two species of nerve are distinguished by a difference of structure. For he maintains the old Galenic doctrine, that the nerves of motion are, as compared with those of sensation, of a harder and more fibrous texture, a diversity which he does not confine to the homogeneous nerves, but extends to the counter filaments of the heterogeneous.* This opinion, in modern times, by the majority surrendered rather than refuted, has been also subsequently maintained by a small number of the most accurate anatomists, as Malacarne and Reil; and to this result the recent observations of Ehrenberg and others seem to tend. (See Memoirs of the Berlin Academy for 1836, p. 605, sq.; Mueller's Phys. p. 598.)

Finally, to the objection—Why has Nature not, in all cases as in some, enclosed the motive and the sentient fibrils in distinct sheaths?—as answer, and *fifth* argument, he shews, with great ingenuity, that nature does precisely what, in the circumstances, always affords the greatest security to both, more especially to the softer, fibrils; and he might have added, as a *sixth* reason and second answer—with the smallest expenditure of means.

The subtilty of the nervous fibres is much greater than is commonly suspected; and there is probably no point of the body to which they are not distributed. What is the nature of their peripheral terminations it is, however, difficult to demonstrate; and the doctrines of Ruysch and Malpighi in this respect are, as he shews, unsatisfactory.

The doctrine of Albinus, indeed, of the whole school of Boerhaave, in regard to the nervous system, and, in particular, touching the distinction and the isolation of the ultimate nervous filaments, seems, during a century of interval, not only to have been neglected, but absolutely forgotten; and a counter opinion of the most erroneous character, with here and there a feeble echo of the true, to have become generally prevalent in its stead. For, strange to say, this very doctrine, is that recently promulgated as the last consummation of nervous physiology by the most illustrious physiologist in Europe. "That the primitive fibres of all the cerebro-spinal nerves are to be regarded as isolated and distinct from their origin to their termination, and as radii issuing from the axis of the nervous system," is the grand result, as stated by himself, of the elaborate researches of JOHANN MUELLER; and to the earliest discovery of this general fact he carefully vindicates his right against other contemporary observers, by stating that it had been privately communicated by him to Van der Kolk, of Utrecht, so long ago as the year 1830 (Phys., p. 596-603.)

In conclusion, I may observe that it is greatly to be regretted that these prelections of Albinus were never printed. They present not only a full and elegant digest of all that was known in physiology at the date of their delivery (and Albinus was celebrated for the uncommon care which he bestowed on the composition of his lectures); but they likewise contain, perdue, many original views, all deserving of attention, and some which have been subsequently reproduced to the no small celebrity of their second authors. The speculation, for example, of John Hunter and Dr Thomas Young, in regard to the self-contractile property of the crystalline lens is here anticipated: and that pellucidity and fibrous structure are compatible, shewn by the analogy of those gelatinous mollusca, the medusæ or sea-blubbers, which are not more remarkable for their transparency than for their contractile and dilative powers.

4. As I have already noticed, the celebrity of the Leyden School, far from commanding acceptance, did not even secure adequate attention to the doctrine of its illustrious masters; and the Galenic theory, to which Haller latterly adhered, was, under the authority of Cullen and the Monros, that which continued to prevail in this country, until after the commencement of the present century. Here another step in advance was then made by Mr Alexander Walker, an ingenious physiologist of Edinburgh; who, in 1809, first started the prolific notion, that in the spinal nerves the filaments of sensation issue by the one root, the filaments of motion by the other. His attribution of the several functions to the several roots—sensation to the anterior, motion to the posterior—with strong presumption in its favour from general analogy, and its conformity with the tenor of all previous, and much subsequent, observation, is, however, opposed to the stream of later and more precise experiment. Anatomists have been long agreed that the anterior column of the spinal marrow is in continuity with the brain-proper, the posterior, with the after-brain. To say nothing of the Galenic doctrine, Willis and the School of Boerhaave had referred the automatic, Hoboken and Pouteau the automatic and voluntary, motions to the cerebellum. Latterly, the experiments of Rolando, Flourens, and other physiologists, would shew that to the after-brain belongs the power of regulated or voluntary motion; while the parallelism which I have myself detected, between the relative development of that part of the encephalos in young animals and their command over the action of their limbs, goes, likewise, to prove that such motion is one, at least of the cerebellic functions. (See Monro's Anatomy of the Brain, 1831, p. 4–9.) In contending, therefore, that the nervous filaments of sensation ascend in the anterior rachitic column to the brain-proper, and the nervous filaments of motion in the posterior, to the after-brain, Mr Walker originally proposed, and still maintains, the alternative which, independently of precise experiment, had the greatest weight of general probability in its favour. (Archives of Science for 1809; The Nervous System, 1834, p. 50, sq.)

5. In 1811, Sir Charles Bell, holding always the connection of the brain-proper with the anterior, of the after-brain with the posterior, column of the spinal chord, proceeding, however, not on general probabilities, but on experiments expressly instituted on the roots themselves of the spinal nerves, first advanced the counter doctrine, that to the filaments ascending by the posterior roots belongs exclusively the function of sensation; and thereafter, but still, as is now clearly proved, previously to any other physiologist, he further established, by a most ingenious combination of special analogy and experiment, the correlative fact, that the filaments descending by the anterior roots are the sole vehicles of voluntary motion. These results, confirmed as they have

been by the principal physiologists throughout Europe, seem now placed above the risk of refutation. It still, however, remains to reconcile the seeming structural connexion, and the manifest functional, opposition of the after-brain and posterior rachitic column ; for the decussation in the medulla oblongata, observed, among others, by Rolando and Solly, whereby the cerebellum and anterior column are connected, is apparently too partial to reconcile the discordant phenomena. (Bell's Nervous System ; Shaw's Narrative ; Müller's Physiology, &c.)

Notice of the Discovery of a nearly complete Skeleton of the Zygodon of Owen (Basilosaurus of Harlan) in Alabama.

By S. B. Buckley, A. M.

Some years ago a few imperfect vertebræ of this animal were sent to Philadelphia, which were found near the Wachita River in Louisiana. These were described by Dr Harlan in 1834, and referred to a lost genus of the Saurian order. From the great size of the bones, he called it the Basilosaurus. Subsequently Harlan obtained other bones of his Basilosaurus, which were found on the plantation of Judge Creagh, of Clark County, Alabama, and forwarded to Philadelphia by that gentleman. These were one or two fragments of the jaws with teeth, of which the upper portion was broken off and lost, also pieces of ribs, with some other long bones belonging to its paddles, and several vertebræ with the processes broken off. These Harlan also described in the "Transactions of the American Philosophical Society." Part of these bones were taken by Harlan to London, where they were pronounced by Owen, from a microscopical examination of the teeth, to belong to a genus of mammalia between the Saurians and Cetacea. He named it the Zygodon, in allusion to the curious form of the molar teeth.

Our skeleton was discovered on the plantation of Judge Creagh, the same gentleman who forwarded the bones already noticed to Harlan, and from the same neighbourhood in which those were obtained. The entire vertebral column is nearly perfect, except two or three of the cervical, which are much broken, and it is possible that others from the same part of the skeleton are lost, since the vertebræ near the head were disjointed and scattered over a surface of several feet, but the remaining portion of the vertebral column was in an almost unbroken series to the extreme tail. *The entire length of the skeleton, including the head, is nearly seventy feet !* Some of the ribs must have been upwards of six feet in length, but of these we only have fragments, including their extremities and central parts. We have also other long bones belonging to its paddles, as the animal was probably an inhabitant of the water. These are small in proportion to the size of the other bones. The principal organ of locomotion of the animal seems to have been its tail, which is short and thick. Many of the dorsal vertebræ are sixteen or eighteen inches long, and upwards of twelve inches in diameter. The transverse processes are from three to six inches long. The spinal and also the lateral processes are of about the same length. These last three are united at the base, where they form an arch through which the spinal marrow ran. This arch, with the lateral and spinal processes, is easily detached from the main body of the vertebræ. The head is much broken, yet we have portions of both jaws with the teeth inserted in nearly a perfect state. The molar teeth are inserted into separate cavities of the jaw by two long roots. The upper portion of these teeth is somewhat hastate, with large and rather blunt serratures on the lower part of the anterior and posterior margins, as in those of the Iguanodon. The average longer diameter of a section of the molar teeth is about four and a half inches. The anterior teeth have a single root, are sharp-pointed, conical, slightly curved, and laterally compressed; the transverse section parallel to the base forming an ellipse. The length of the anterior teeth, including the root, is five or six inches, and the longest diameter nearly two inches. The form of the molar teeth is so peculiar that it is impossible to give a correct idea of them without the aid of plates.

The Zygodon or Basilosaurus was imbedded in a marly limestone soil. The upper portion, to the depth of one or two feet, is a rich black vegetable mould. Beneath this is a yellowish white marl, yielding easily to the mattock, and containing few organic remains. Most of the bones were in this marl from one to six feet beneath the surface. At the depth of about six feet is a green sand or marl resembling the green sand of New Jersey, and containing few organic remains. The vertebral column, as has been before remarked, lay in an almost unbroken series from the head to the extreme tail, and appeared to occupy the place upon which the animal died. The bones are more or less fossilized, having lost nearly all the animal matter, and been penetrated by carbonate of lime. Yet a large portion of their surface retains the smooth and ordinary appearance of bone. The enamel of the teeth is also retained. Numerous sharks' teeth and shells are scattered over the surface, or imbedded in the soil. The most common of the fossils are several species of the genera Ostrea, Exogyra, Pecten, Echinus, Conus, and Scutella. The rocks of the immediate vicinity are limestone, which is sometimes as white and nearly as soft as chalk, but destitute of flints or organic remains. This variety is often sawed into blocks and used for building chimneys. The rock partly surrounding the field in which our bones were discovered, is a white limestone filled with nummulites. The grey limestone, with more or less organic remains, is the prevailing rock of the immediate neighbourhood. These limestones often present an almost perpendicular escarpment of rock, sometimes in the form of little islands, against which the waves of the olden time appear to have dashed. The spot seems once to have been an estuary or arm of the sea, interspersed with small islands, and here the Zygodon appears to have lived. Bordering the limestone within a mile of this place, is a red sandstone apparently destitute of organic remains. This forms the most elevated part of the country, and extends over a large portion of Clark County, affording a poor soil, of which the prevailing timber is the long-leafed pine (*Pinus palustris*) associated with dwarf oaks. This sandstone often affords hollow cylinders several inches in diameter, and from one to three feet long, the cavities of which frequently contain a red ochre (oxide of iron), sometimes used by children as a pigment.

Bones of the Zygodon have been seen in Washington County, Mississippi, and from thence they have been found in several places as far east as Clariborne, on the Alabama River. Judge Creagh relates, that when he first moved to Clark County, about twenty years ago, these bones, consisting mostly of large vertebræ, were so numerous as seriously to interfere with the tillage of some of his fields; and hence they burned large quantities of them in the fires of their log heaps. At this time scattered vertebræ, generally much broken and wanting processes, are lying on the surface of the ground in almost every field of Judge Creagh's and the neighbouring plantations. Among these no head or part of one is known to have ever been seen, except those parts which Dr Harlan described, and these in our possession. The reason of this is, that the jaws were hollow and composed of a thin plate or plates of bone filled with animal matter; and when this matter contained in the cavities was destroyed, the exterior plates were easily broken. It may be well to mention that Clark County is situated between the Alabama and Tombigbee Rivers, about one hundred miles north of Mobile.

For a knowledge of this huge being of a former and remote age, the public are greatly indebted to Judge Creagh, who kindly assisted and furnished hands to assist in digging out the bones, and provided materials to make the boxes for containing them. He also had them conveyed to the Tombigbee River (twelve miles), after which he refused to take any compensation from one who went to his house a stranger, without even a letter of introduction. The skeleton was sent *via* Mobile to New York, where it is at present in fourteen large boxes, some few of which have been opened to gratify the curiosity of several scientific gentlemen, who are ready to testify that we have a unique and veritable skeleton of the Zygodon.*

On Artesian Wells. By Admiral Sir David Milne, G.C.B. Communicated by the Author.

The theory of Artesian Wells, maintained by Buckland, Arago, and other first-rate authorities, rests on a circumstance, which seems to me liable to some doubt, and which I now mean briefly to consider, namely, the necessity of the water which ascends through the bore, having previously descended or filtered through the earth, from some higher ground in the neighbourhood.

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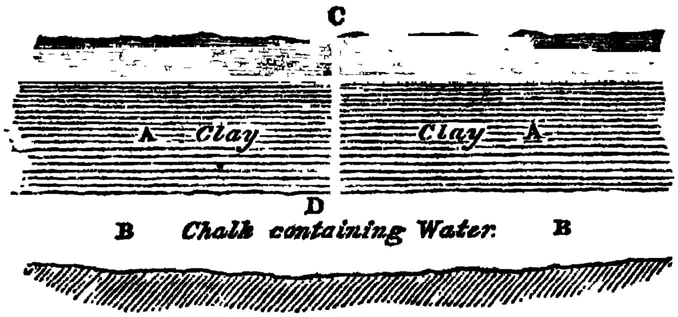
* American Journal of Science, April 1843, p. 409.

But it is a well known fact, that in the vicinity of many artesian wells there are no hills at all, nor any particular elevation of the earth's surface; and the supporters of the now existing theory are driven to the necessity of seeking for these heights at any supposable distance. M. Arago, indeed, himself says, "that they must be sought for even beyond the sphere of vision, at the distance of forty, eighty, or a hundred and eighty miles, or even more, if necessary."

Many years ago it struck me, that a much more probable and less complicated theory of artesian wells might be maintained, and which does not imply the necessity for there being any higher ground in the neighbourhood, none indeed, at all higher than the mouth of the tube inserted in the bore for the supply of the springs.

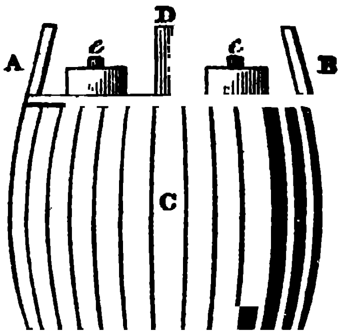
To elucidate my theory, let us suppose, immediately under the surface where the bore is made, a superincumbent mass of clay—such as the London clay—lying over chalk, or other formation, containing water. A diagram will more clearly shew this.



A denotes the bed of clay, a pipe is made to penetrate it from C to D, where it reaches B, a bed of chalk containing water. On known hydraulic principles it is demonstrative, that the instant the pipe C D reaches the stratum B containing water, the water will be immediately forced up the pipe from the pressure of the superincumbent mass of clay, and will rise to the surface of the earth at the point C.

As another illustration, let us suppose that the bed of clay rested nearly horizontally on the lower strata containing water, and it will be seen that the same result would follow. The moment that vent was given by the bore to the water, it would be forced by the superincumbent weight to rise in the tube. The superincumbent strata will, as the water flows up, sink down, and continue to force up the water, so long as any remains at the bottom of the bore. Indeed, the same reasoning applies to the continuance of the supply of water, even if we admit it as coming from a higher level. To support the latter theory, however, we have seen that it is necessary to admit the dubious fact, of the required water coming from heights sometimes at almost inconceivable distances; whereas, by the former, this difficulty is entirely got over.

The more distinctly to illustrate this, let us suppose a cask or wooden box C filled with water, until it reaches the cover A B, which is tightly fitted as a top. Then insert a hollow tube D through the cover A B. When the pressure of the cover is increased by the addition of the weights *e e*, the water will rise to the top of the tube D, and it will continue to overflow as long as the pressure is made, or the supply continues, The same principles apply exactly to the rise of water, by superincumbent pressure, from the bed of clay. If it be argued against this theory, that the supply of water would in time cease, it may be answered, that the same argument obtains, and with equal force, against the other; for whether the water comes from a higher level, or only from a free level, it should be remembered, that it has been admitted by the best authorities that artesian wells are sensibly affected by long-continued droughts.



In confirmation of these views, and as a familiar practical illustration, I may be permitted to mention, that when the present basin was being constructed in Portsmouth dockyard in 1796 or 1797, the piles first driven were 15 feet in length. From their length, these passed entirely through the bed of clay, into sand containing water. The consequence was, that the water squirted up in all directions from the subjacent reservoir, and as it was uncertain to what extent the piles would sink before they reached any solid stratum, it was resolved to shorten the piles, so that they might not pass through the clay at all; and the old holes were with some difficulty plugged up. Some years afterwards, the dockheads of one of the principal docks gave way, and sunk considerably; nor could any of the officers of the yard account for the occurrence. It happened at this very time, that, with the late Commissioner Sir George Grey, I was examining the docks; and recollecting the circumstance which rendered it necessary to cause the piles to be shortened, namely, their having penetrated into the sand containing water, it occurred to me to ask Sir George if any wells were at the time being sunk in the neighbourhood? His reply immediately was, that "certainly workmen were then sinking for water near the officers' houses." Upon which I remarked, that if the sinking of the well was not stopped, other parts of the masonry of the docks would most assuredly give way; and that no wells should be allowed to be sunk near the dockyard, as, by giving vent to the water in the sand under the clay, the foundations of the buildings must necessarily be affected. This hint was, I believe, attended to, and the sinking of the well was stopped. In situations such as these, the insertion of a pipe through the clay would afford the supply of water with much more safety, the drain being less, and much more gradual.

Indeed, I can perceive no difficulty adhering to the theory of artesian wells now propounded, which does not, with equal force, adhere to the other; and it is free from many objections to which it is liable. When we read in Dr Buckland (p. 560) when treating of this subject, that, "by similar wells, it is probable that water may be raised to the surface of many parts of the sandy deserts of Africa and Asia," we may be well tempted to exclaim, "And where is the level there from which it can come?"

If the theory I have suggested be well founded, it must be obvious, that in a great town, or the neighbourhood of it, built on a mass of clay, through which wells are sunk, there will be a constant, though probably very slow and gradual, subsiding of the surface. Such situations would, therefore, be ill adapted for permanent observatories for astronomical purposes; as a subsidence of the ground, inappreciable on the earth itself, may produce sensible errors in observations of the heavenly bodies. May not some of the anomalous results obtained at observatories be ascribed to this cause?

Remarks on the Geology of the Island of Little Ross, Kirkcudbrightshire. By THOMAS STEVENSON, Esq., Civil Engineer. Communicated by the Author.*

Having been for some time resident on the island of Little Ross, whilst superintending the erection of a sea-light and other works on that coast, I had an opportunity of examining the geology of the island; and as my observations have led to results which, to myself at least, were unexpected, I have ventured to draw up a short account of them.

Little Ross is situated at the mouth of Kirkcudbright Bay, and is about 1500 feet long by 800 feet broad. Its distance from the nearest point of the mainland is not more than 340 yards; and although there is a depth of about 20 feet at high water, there is nevertheless a narrow ridge by which, in many low spring tides, it is possible for foot passengers to cross over, not, however, dryshod.

The district in which this island is situated has been generally supposed to exemplify the Cambrian group of the Transition class; and accordingly the Little-Ross island, at first sight, presents the usual appearances of the greywacke of that geological epoch. It exhibits also a singular scene of disruption, torsion, and upheavings of the strata, which attain a height of 100 feet above the level of the sea. At the north-western extremity, or that most sheltered from the sea, there is a beach called the White Bay, consisting of shingle; while at all other points the rocks expose surfaces varying from low angles up to the perpendicular. There is but a thin covering of sod over the surface, below which may be found angular fragments of the mouldering rock, embedded in loam or earth resulting therefrom; and lying on this are erratic boulders of granite, compact felspar, and porphyry, as well as a few water-worn pebbles of greywacke.

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* Read before the Wernerian Natural History Society, 8th April 1843.

The rocks themselves* consist of beds of greywacke, alternating with conformable greywacke slates, or, as they are there termed, *Slate-band*. The greywacke beds vary in fineness, from the coarsest conglomerate or breccia, up to a nearly homogeneous blue and sometimes greyish rock. And first, I may notice the composition of the conglomerates, which consist of water-worn boulders of greywacke, embedded in a softish matrix of the same rock, or at least in an argillaceous rock of very similar description. This may perhaps favour the conclusion that these deposits belong to the more recent of the transition class. With regard to the breccia, it is proper to state that it should more strictly be termed a *breccia-conglomerate*, as it consists of embedded fragments, partly angular, partly rounded. These conglomerates are often permeated by veins of yellowish, and sometimes red carbonate of lime, tinged probably by manganese.

Next in fineness comes a very coarse greywacke, consisting of small pebbles of quartz, fragments of jasper, Lydian stone, pieces of clay-slate, &c. This rock is pervaded with *dries*, which separate it into triangular prisms. The other greywackes have all a rhomboidal fracture (of from 70° to 80° obliquity), and are of various degrees of fineness, and invariably shew specks of mica. They are all more or less cut up by dries or cutters, and are also pervaded in every direction by what appear to have been cracks or splits, which are occupied by carbonate of lime, tinted red. These cracks, some of which intersect each other, give the rock, when cut for building purposes, a beautiful variegated or veined appearance. Besides these, there are brittle shales, which the action of the weather has in many places reduced to a mass of disconnected prismoidal fragments, that moulder away into a ferruginous earth. The regular beds of greywacke are of two kinds—the one blue, the other whitish, and liable to weather red, so as to resemble, externally, beds of red sandstone. These two kinds seem to alternate with *slate-band* possessing corresponding characteristics. The blue alternates with dark-blue slates, having glossy sides, and of a fine and sometimes silky texture; while the whiter variety alternates with greyish-white slates, which weather still whiter.

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* For a description of the rocks found in the Stewartry of Kirkcudbright, see the writings of Professor Jameson and of the late Mr R. J. H. Cunningham.

Several of the slates have the peculiarity of unconformable cleavage-planes. The angles which the planes of cleavage make with the line of stratification, are generally about 30 degrees.

In some of the rocks above described, I have found embedded small pieces of *anthracite*. Quartz-veins do not occur, but carbonate of lime is very common; and sulphate of barytes is not unfrequent. In one or two instances, I have found in crevices, small quantities of *bitumen* or *mineral pitch*, galena, and, in one instance, steatite, which shall be afterwards described. In some parts also there are traces of copper-ore.

These constitute all the stratified rocks which this small island affords. Of trap rocks, we have several varieties. At the beach called White Bay, and at the top of the island, there are dykes of reddish coloured greenstone, in some places passing into a granitic rock; while, at others, they resemble a compact felspar rock. Besides the greenstones, I have seen two dykes of compact felspar base, with here and there crystals of hornblende or augite interspersed. In a small felspathic dyke, on the south side of the island, there are brilliant specks of iron-pyrites.

In contact with these igneous intruders, the strata have undergone the usual changes. The greywacke has become crystalline, so as to resemble greenstone, and the slates have become, in some cases, a sort of Lydian stone. In others, they are converted into flinty slates, of a yellow or cream colour, and in both the edges of fragments are translucent, and the fissile property is destroyed. Near the trap-dykes the *altered rocks*, in some instances, contain specks of iron-pyrites.

Such, then, are the rocks of which the island is composed; and the characteristics which render them interesting, are the following.

On the eastern side, close to the sea, at a place where stones were quarried for the lighthouse works, there is a range of rocks tilted up at a high angle, and of which the quarrying operations have afforded a section. Among these is a bed, the upper side of which appears to have been exposed to diluvial action at some time posterior to its consolidation, as the surface is uneven and rounded. Conformable with this, and superior to it, is a series of very thin ($\frac{1}{16}$ inch) layers of slate-band. These layers of slate have obviously been deposited after the lower bed had been wasted, as they not only rest conformably with it, but have followed all the salient and re-entrant angles which its water-worn surface presents. This renders it probable that some considerable period had elapsed between the deposit of this member of the series and the incumbent shales.

In the finer sorts of greywacke, I found very good specimens of the *graptolite* or *sea-pen*. The off-shoots or feet are visible on one side only, so as to give the fossil very much the appearance of a saw. I have also found one specimen of the double graptolite, such as was observed in Norway by Dr Beck of Copenhagen.

On the western side of the island I found a quantity of steatite investing the irregular masses of the whiter sort of greywacke; nor does it traverse the rock as a vein, but seems confined to one spot. Some parts of it are translucent, of a greyish-yellow colour, with a shining oily lustre, and streaked appearance externally; while other parts are of a very dark green, or blackish colour, and massive.

In the coarser greywacke, formerly described as a closely aggregated rock, containing a variety of fragments and pebbles, my friend Mr J. T. Syme, civil-engineer, who spent some time with me on the island, found part of a shell; and I have since then procured many specimens that appear to be of different species; but I have only found one entire *shell*, which was unfortunately lost soon after I had got it. So far as I could judge from the imperfect examination that I had an opportunity of making, it appeared to belong to the genus *Terebratula*.

In the same sort of rock, which is certainly the last, both from its structure and the coarseness of its texture, that any one would think of examining for organic remains, I have observed small circles of carbonate of lime, having a black spot in the centre, with apparent radiation, which on the whole closely resemble fragments of the stalks of Encrinites. But as I have seldom met with this appearance, I am not prepared to state that *Encrinites* are to be included in the list of the organic remains of the island. It is important to remark that the rock where these occur bears no mark of being stratified, having no cleavage plane, and being every where pervaded by drics, which separate the mass into triangular prisms, and in some cases into other symmetrical solids. The occurrence, however, of organic exuviæ alone, independent of the rounded pebbles which it contains, are, of course, absolutely conclusive as to its being of mechanical origin.

Mr E. G. Fleming of Kirkcudbright showed me a fossil which he had found several years ago in the solid greywacke rock, at a point on the mainland nearly opposite to Little Ross Island. This fossil I found to be an *Orthoceratite;* and another specimen has since then been found in the same spot, which confirms its identity.

There remains one point of interest to be mentioned, and this is the occurrence of an *elevated sea-beach*. As formerly noticed, there is but one beach on the island, at a place called the White Bay. It consists of oval-shaped shingle, of a size which, in Lancashire, would be called *single stanner*, and it is principally composed of greywacke, with here and there travelled boulders of porphyry, of various colours. This shingle has assembled itself into two little bays, with a connecting spit between, the top of which is covered with lichen, and it is composed of the same sort of shingle as the beach below, but is considerably above the range of the highest tides. At the top of this spit the grass line occurs, and to appearance the beach here seems to end, but in reality the boulders are found in a decomposing state, and bedded in the loam resulting from that decomposition, for at least 20 feet above the present highest tide-mark. The grassy part has also the same slope and contour as the uncovered part of the beach. In cutting through this bed for a road, I found, at a depth of 4½ feet below the surface, *cockle*, *periwinkle*, and other shells, which still inhabit the neighbouring shores.

Here, then, is a beach whose level and appearance prove the gradual elevation of the island. Unlike many other similar relics of the sea that are found without anything intermediate to substantiate the connection, this beach presents an uniform continuation of the existing shore to the height of 20 feet above the range of the highest tides, and thus would seem to afford strong presumptive evidence, that the land at this place does, or at least did, *gradually* emerge from the ocean, the general shape and contour of the present bays being easily recognisable on the grass-grown land.

I am sensible that the foregoing observations are in some respects but very imperfect, yet I feel justified in giving this sketch, as the facts which happened to come under my notice seem to favour the conclusion that the Silurian system, as characterized by Mr Murchison in his work on the Silurian System, exists in Scotland.

On the Sexes, Organs of Reproduction, and Mode of Development, of the Cirripeds. Account of the Maidre of the Fishermen, and Descriptions of some New Species of Crustaceans. By HENRY D. S. GOODSIR, Esq. Communicated by the Author. With three Plates. No. V.

SECT. I.—ON THE MALE OF THE BALANUS.

It has hitherto been a question whether the sexes in the Cirripeds are distinct as male and female, or are combined in the same individual; and the opinions which have originated from this, are both very numerous, and at the same time contradictory. No two authors agree as to the anatomy and physiology of the generative organs of these animals. The various and contradictory nature of the opinions regarding this question in relation to the organs of generation, is sufficient to cast a doubt upon the correctness of any one of them. The Cirrhipeda have, until lately, always been looked upon as mollusca, and this is, without doubt, the cause of so much confusion and uncertainty.

Mr Hunter, who was the first author that examined the anatomy of these animals with any degree of care, states his opinion concerning the organs of generation in the following terms. "It is most probable that all Barnacles are of both sexes and of the first class, viz. self-impregnators, for I never could find two kinds of parts, so as to be able to say or even suppose the one was male the other female." He describes what he supposes to be the tubular portion of the testicle, the vasa deferentia, and the penis, but at the same time makes no mention of an ovary. The erroneous conclusions to which Hunter came in regard to the true nature of these organs, must have arisen from the circumstance of his having examined unimpregnated specimens only.

Cuvier has the same opinion as to the hermaphroditic characters of the Cirripeds, but differs from Hunter as to the anatomy and physiology of the generative organs. What Hunter supposes to be the testicle, Cuvier considers the ovary; the vasa deferentia as the oviducts, and the penis as the ovipositor; at the same time stating his belief that the ova are impregnated as they pass through the ovipositor. He was led to form these opinions from finding what he considered to be the ovary filled with small granules which he supposed were ova.

Some other authors again, along with whom we find Sir Everard Horne, consider that these organs already spoken of, and thought by Hunter and Cuvier to be the only organs of generation, are merely the male organs, and that the ovary is situated in the peduncle; also that impregnation takes place by means of the organ which Hunter termed the penis. This opinion relative to the existence of the ovaries in the peduncle of the animal is incorrect. The mistake has evidently arisen from the ova being found in this part of the body after their escape from the ovaries. They are deposited in this place by the ovipositor until they are sufficiently matured to be expelled from the body of the parent animal. Likewise, there is no appearance whatever of any glandular structure in this part of the animal, which could be adduced in support of this opinion.

In looking over these various opinions, we find that Cuvier's is the one which approaches nearest to fact in regard to the female organs of generation. If a common Balanus (*Balanus balanoides*) is taken from the rock during the month of April, and the enclosed animal examined, the oviducts, or what Hunter termed the vasa deferentia, will be found filled with an immense number of minute yellow granules. These are the ova. After a period, they pass along the oviducts and ovipositor; the organ which Hunter termed the penis; and are introduced in this way into the interior of the shell, or rather into the cavity which exists between the body and mantle of the animal. The ova are arranged in irregular layer-like masses at the bottom of this cavity, and sometimes it is completely filled with them. At this season the ovipositor is constantly bent downwards and inwards along the right side of the body of the animal. The ova, when within the oviducts, as has been already stated, are of a globular shape; but as they escape, or shortly after they are lodged in the cavity of the mantle, they assume an ovoid shape, sharper at the posterior extremity than the anterior. When sufficiently matured to be expelled from the body of the mother, which may be either immediately before or after the young animal bursts through the ovisac, they are carried out in successive currents at each retraction of the cirri.

From the above statements it will be seen that Cuvier's opinion as to the nature of the granules which he observed in the ovary was correct, viz. that they were ova. So that the organ which Hunter considered as the male secreting organ, is in fact the ovary. The only other part, then, which could act as an impregnating organ, was the tubular proboscidiform organ, which, according to some authors, has a glandular structure near its base, and which is considered by them to be analogous to the testicle. Nothing, however, will be found in this organ having a glandular appearance or structure, so as to allow us to maintain this opinion.

From the above observations, then, it will be seen that the animal which has heretofore been considered as a hermaphrodite, has organs of generation essentially female, and that impregnating organs are altogether awanting. The conclusions, therefore, which we are bound to draw from these observations, are, first, that the Cirripeda are not hermaphrodites, that the sexes must be separate; and second, that the male must exist as a separate and distinct individual.

Mr J. V. Thompson, whose opinion is of the greatest weight in regard to the history of these animals, says, when speaking of the small crustaceous-like animal which he afterwards found to be the larva of the Balanus,—" Circumstances induced a belief that they were the larva or disguised state of some crustaceous animals, or, (as it had been previously ascertained, that the Cirripedes were crustacea), that they were the *males* of these, not being disposed to believe that the two sexes were united in the same individual. In favour of this idea, too, it may be observed, that the males of many crustacea are remarkably less in size and different in aspect, as in the Caligi and Bopyri, and also that in some they are rarely met with, and only at a particular season." Again, the same author says, " From a consideration of the whole history of these animals, are we to conclude that they have the *sexes united?* A fact so much at variance with what we see in all the rest of the Crustacea may authorize a degree of scepticism."

Having then satisfied myself that the Cirripedes were not hermaphrodites, and seeing, at the same time, that, in as far as the young or larva were concerned, these animals were really crustaceous, and having also these statements of Mr J. V. Thompson's before me, I was led to suppose that the sexes were distinct, and that the male animal would be found to resemble that of the lower Syphonostomous Crustacea, such as the Lernæa, &c.* The male of the Lernæa is always found attached near to the external oviducts, and in some cases upon that part of the body in which the ovary is situated, as in Anchorella uncinata. This being the case, it was thought that the male of the Balanus would be found in an analogous position. Under this supposition the ovipositor was carefully examined in a very great number of cases, at all seasons of the year; but nothing in the shape of a separate animal could be observed.

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* Professor Edward Forbes of the King's College, London, in a course of lectures on Zoology delivered by him in Edinburgh during the years 1840–41, drew an analogy between the Lernææ and the Pedunculated Barnacles, in so far as regards the external oviducts of the former, and the pedicles of the latter, both of these organs being considered by him as parts of the organs of generation,—receptacles for the purpose of bringing the ova more safely to a state of maturity.

During the beginning of the month of May (1843), however, while engaged in examining specimens of the *Balanus balanoides*, in the hopes of still finding my supposition correct, a small fleshy body was observed, not on the ovipositor, but on the body of the animal, immediately over the ovaries. This body was adhering with a considerable degree of firmness, and on being placed in a separate vessel of sea-water by itself, it was found to be alive, and to bear a great resemblance in its external appearance to a Lernæa.

On making a more minute examination, the anterior part of the body was found to be minute and crustaceous, consisting of six articulations. The eyes are two in number, black, shining, and pedunculated. The antennæ are four in number, and are generally in constant motion. Owing to the apparent disproportion of the two divisions of the body, this animal is entirely unfitted for locomotion; but the crustaceous or anterior division is constantly moving backwards and forwards.

We shall proceed, then, to describe in detail this animal, being of opinion that it cannot be other than the male of the Balanus. (Plate IV. Fig. 10.)

The whole animal is of a straw colour, the anterior or crustaceous portion being of a lighter shade. The body, as already stated, consists of two parts, an anterior and a posterior; the former of which is minute, crustaceous, and composed of six articulations; the latter is unarticulated, large, fleshy, lobulated, and contractile. It has also a number of fleshy extremities, which apparently represent feet. A long, fleshy, tail-like appendage also arises from the mesial line posteriorly. The anterior part of this portion of the body is trilobate, and projects above and beyond the crustaceous portion of the body, altogether concealing it from view when the animal is lying in the natural position.

The first segment of the crustaceous portion is the largest of all the six, and is of a semicircular shape. It supports the masticatory apparatus, two pairs of antennæ, the two eyes, a pair of strongly pectinated organs, and a pair of long, sharp, claw-like members. (Plate III., Fig. 3, *a.*)

The eyes are large, shining, black, and pedunculated, and, as far as could be made out, were to a certain extent mobile.

The first or anterior pair of antennæ are each composed of a single large, flat, scale-like joint, which has its extremity armed with seven or eight long, delicate filaments, the two first of which are biarticulate. Each of the external antennæ consist of nine joints, the two first of which may be considered as peduncular; the last seven are much more delicate and slender, and have each of them a spine at the distal and anterior extremity; the ninth has the extremity armed with two or three long and very delicate spines.

The mouth is situated at the posterior part of this segment. It appears to be suctorial; but, from the extremely minute size of this portion of the body altogether, this has not as yet been made sufficiently out.

A very strongly pectinated scale arises from the base of the first pair of antennæ, one on each side. These arise almost from the mesial line, and cover the anterior antennæ. The posterior edge is armed with seven or eight long, sharp, and powerful teeth. Another pair of strong claw-like extremities arise from the base of the anterior antennæ, which last are directed backwards.

The feet are ten in number, five on each side. Each of them consists of six articulations, the last of which is armed with a strong terminal claw. The first, second, and third pairs are rather short, and have the last articulation spherical. The fourth pair are large and powerful, but the fifth are much more slender. The extremities are apparently unfitted for locomotion, and are therefore generally bent in upon the abdominal surface of the body, except the last pair, which appear to be constantly moving about.

The four middle segments of the body have their external edges inflected beneath the body to within a little of the mesial line. The posterior edges of these inflections are strongly pectinated, after the same manner as the organs at the bases of the first pair of antennæ. These are, doubtless, for the purpose of retaining a firm hold of the female during the act of copulation.

The external organs of generation are situated at the base of the last pair of feet; they are articulated, and a slender vessel, the vasa deferens, runs from the base of each round to the dorsal aspect of the segment to the testicle, which is probably situated in the soft portion of the body. (Plate III., Fig. 6, *b*).

The soft portion of the body consists of three parts; these are separated by means of neck-like contractions, which divide the portion into three equal sections. The first of these is trilobate, and has been already described. The second has two arm-like extremities arising from each side of it, representing, as it were, anterior extremities. These run backwards, and taper very gradually to a point. The third or last division of this part of the body has also two extremities of the same appearance as the last, together with a third tail-like extremity, which arises from the mesial line, and lies between the two last described.

In looking over the above description, we cannot fail to see the points of affinity between it and the larva described by Mr J. V. Thompson, and which are of considerable importance, such as the pedunculated eyes, &c. It has also many connecting points with other crustacea—to the Lernææ by means of its soft fleshy body, and to the higher crustacea by means of its pedunculated eyes and antennæ.

From the researches of Mr Thompson relative to the metamorphosis of the Cirriped, there can be little doubt of the relation which they bear to the Crustaceans. There was only one point which could make this relation at all doubtful, viz., the hermaphroditic character of the Cirriped, seeing that one of the great fundamental distinctions between the higher and lower Articulata, is the separation of the sexes in the former, and their combination in the latter. From this circumstance, it would have been impossible to have admitted of the junction of the Cirriped and Crustaceans, which had heretofore been considered as belonging to two separate classes.

Looking upon the above described animal, then, as the male of the Balanus, we completely break down this only objection, a circumstance which must now entirely decide the question as to the crustaceological character of the Cirriped.

As to the objections which may be raised against this animal being the male of the Balanus, it may be asked, why has it not been observed before? This question may be met in more ways than one. It is a fact consistent with our knowledge of the Crustacea, that the males of many species become visible during certain seasons only; also, that one impregnation is sufficient for several generations. Now, these very facts are known, and have been proved to take place amongst the very species of Crustaceans with which the Cirriped must be arranged. These facts must serve to strengthen our opinion as to the natural arrangement of the Cirripeds in the animal kingdom.

The male of the Balanus becomes visible, without doubt, at certain seasons only. During the season of love, the posterior portion of the body which seems to contain the organs of generation, is much enlarged; and after the act of impregnation has been fulfilled, these organs become atrophied for a season. Looking upon this suggestion, then, as the true one, it cannot be wondered at that the anterior part of the body, which is so very minute, should escape observation within the body of the female animal. It is also a curious fact, and supports the above suggestions, namely, that in those specimens where the ova have reached the mantle, the male is not seen, having only been found in those as yet apparently unimpregnated. Since there are many points of similitude between this animal and others of the Crustacea, it may be well to direct attention to some of these.

To the Lernææ, in general, it has many points of connection, but to none so nearly as to those belonging to the "Tribes des Ergasiliens" of M. Edwards, and to the only species of the genus Nicothoa of that tribe it has a most striking likeness. The Cirripeds, however, are analogically connected to this tribe in many respects. The larvæ of the Lernææ and Cirripeds resemble one another very closely in their internal as well as external structure. The larvæ of both are free animals; but as they arrive at maturity, the females become permanently fixed and monstrous, and the males are attached to the body of the female, upon or near to the organs of generation. The organs of locomotion are situated round the mouth, and to a considerable extent act as organs of respiration. The young of both are endowed with organs of vision, which in after life become extinct—in all cases in the Cirripeds, and in almost every instance in the Lernææ.

These facts will show the near connection which the two sets of animals have to one another, and that it is a more close relation than that of mere analogy which exists between them. In fact, the Cirripeds may be considered as Lernææ, the former being attached to animate, the latter to inanimate bodies. This last assertion, however, is not altogether correct, inasmuch as we find species of Cirripeds attached to the skins of the Cetacea.

There are some other points of similitude between the male Cirriped and the Crustaceans, but these are of less note. It is connected to the Podothalma by means of its eyes; and to the Isopoda by means of the structure of its feet, and the anterior division of its body.

The natural size of this animal is (when the organs of generation are at their highest state of enlargement) about a line in length, and a line in breadth. In some cases, however, it is rendered much larger from being infested with a parasitic crustacean. This parasite is an Isopod belonging to the family *Ioniens* of M. Edwards, and which will form the type of a new genus in that family. (Plate III., Fig. 7).

Of this parasite, we shall now proceed to give a short description. It infests the soft part of its victim only, and sometimes in very considerable numbers. When brought under the field of the microscope, it was found to belong to that section of the Isopodous crustaceans, termed by M. Edwards, the Sedentary Isopods. It is about the fourth of a line in length, almost colourless, except in the middle of the body, where it is of a dark brown colour. The body consists of seven segments, of which the second appears to be the longest; this one, however, seems to be also articulated, although very indistinctly, and is probably composed of five segments instead of one, seeing that the five pairs of legs arise from this part of the animal's body.

A long triarticulate antenna arises from each side of the first segment near to its posterior and external angle. The two first joints are the thickest, and both of them conjoined are almost equal in length to the third, which has its extremity armed with two spines.

Five pairs of very short, but thick and powerful, legs arise from the following segments or segment:—

Each of them is three pointed; the first joint is thick and short, the second much more slender, and the third or last is spherical, armed at its extremity with a small claw.

From each side of the six following segments of the body, there arises a long flattish scale, each of which scales has its extremity armed with two, three, or four long filamentous, slender, spines, which are quite stiff and directed backwards. The two last, or terminal of these scales, are the strongest, and also the longest, the spines gradually decreasing in length as they approach nearer to the anterior extremity.

When this animal is taken from its natural habitat, its motions are very feeble, and it is apparently quite helpless. The organs of sight seem to be awanting, or are very minute, the habits of the animal being such as not to require them. The ova are large, and gradually lengthen out into the form of a double cone, as the animal is ready to burst through the ovisac.

SECTION II.—ON THE LARVA OF BALANUS BALANOIDES.

During the greater part of the months of April and May of this year (1843), the water around the Island of May has been darkened with innumerable shoals of the young of the common Balanus. These Cirripeds adhere, in vast numbers, to the sides of the precipitous rocks beneath low water-mark.

A number of these were taken from the rocks, and those which were found to be loaded with spawn were put into a separate vessel by themselves, and the water regularly changed upon them until the young animal escaped. In this way their development and structure was made out satisfactorily.

In those where the ova had but recently escaped from the ovaries, these were closely packed together in the bottom of the cavity of the mantle, in large and firm layers; but as they became more matured, these masses became more broken up and disconnected, and gradually disappeared as the young were expelled from the body of the mother.

The appearance put on by the ova, shortly before the young animal has burst from the ovisac, is that represented in Pl. IV., fig. 13; it is semitransparent, and the motions of the animal may be observed through the membranes.

The larva, shortly after its escape from the ovum, is represented in Pl., IV., fig. 15. The body is of a pyriform shape, being large and rounded anteriorly, while it is small and pointed posteriorly. It is almost colourless in some parts of its body, but the general shade is dark brown, which is deepest in the centre.

The eye is large, of a black colour, is situated in the mesial line, near the anterior edge; it is of a quadrate shape. Almost in a line with the eye, and from the lateral edge of each side of the body, there arises a short horn-like process which curves slightly forwards.

The body is composed of a number of segments, which are most numerous at its posterior extremity; the last segment is armed with three sharp strong spines which project backwards.

This animal has three pairs of extremities, the first of which is single, and the other two are double. The first extremity is composed of a greater or less number of long spines. The two following pairs of extremities are each double, or composed of two parts, which arise from a common peduncle. The anterior part is articulated, but the posterior consists of one piece only, which is almost as large as the anterior or articulated portion. Each of these divisions is armed with a great number of long spines. The third or last extremity is formed in the same way as that last described, but is not so large.

After the Balani have been about a week in existence, they put on the appearance represented in Pl. III., fig. 8. They have changed in their appearance to a very considerable extent, are rather larger and much more active in their habits, than those which had escaped from the ovum more recently.

A large segment has originated at the anterior part of the body, and the horn-like processes described formerly, now form the posterior and external angles of this segment; it also supports the first pair of feet, which are constantly directed forwards, and are now apparently composed of three segments only.

The eye is large, shining, black, and quadrate.

The remaining part of the body is pyriform, composed of several segments, and having the two remaining extremities arising from each side of it. The tail consists of two long spines, which arise from each side of one of the last segments of the body. These spines are almost equal in length to the body, and they are strongly serrated on their external edges. The first pair of legs arise from the anterior part of the first segment of the body; they consist of a peduncle, which is composed of two or more segments, and of two separate portions which arise from the peduncle, and which are formed in the same way as those of the former stage. The last pair of legs is also formed in the same way.

SECTION III.—ON THE LARVA OF BALANUS TINTINNABULUM.

There is no set of animals which has caused greater annoyance to systematists than the Cirripeda.

They were first arranged by Linnæus, along with the testaceous mollusca. Cuvier at first followed this arrangement; but latterly placed them in a distinct class by themselves between the Mollusca and Articulata. Lamarck, Latreille, M'Leay, and other authors followed this latter arrangement; the two last authors acknowledging, at the same time, their closer connection with the Articulata.

The decision of this important question, however, was left to our countryman, Mr J. V. Thompson. This gentleman having obtained some minute mussel-like animals, at first considered them to be nondescripts belonging to the Crustaceans, but on a further examination, and by keeping a few of them alive in glass-vessels of sea-water, he was soon enabled to make out their nature and relations satisfactorily. To use Mr Thompson's own words—"They were taken on the 1st of May, and on the night of the eighth the author had the satisfaction to find that two of them had thrown off their exuvia, and wonderful to say, were firmly adhering to the bottom of the vessel, and changed into young barnacles." The above mentioned statements set at rest, in a great measure, the previous discussions as to the position of the Cirripeds in the animal kingdom.

In the beginning of March of the present year (1843), while Professor Reid of St Andrews and myself were watching the movements of some very large balani (*Balanus Tintinnabulum*), we observed a few of them ejecting with considerable force a great quantity of small granules every time the cirri were retracted. No great attention was paid to this at the time. Next day, however, we were astonished to find the basin in which the balani were confined swarming with an innumerable number of extremely minute but very active animals, when it immediately struck us that these must have been the young which the balani were throwing off the day before. On placing one of these animals under the microscope, we expected to find one of these mussel-like animals described by Thompson; but instead of that, it had an almost exact resemblance to the young of the genus *Cyclops*. To make sure that there had been no mistake, one of the adult balani was opened, when the large cavity of the mantle was found to be filled with the granules which we had formerly seen ejected. A few were placed in a watch-glassful of sea-water under the microscope. They were quite motionless, of an ovoid shape, sharper at one extremity than the other (Pl. IV., fig. 12). The eye, or rather what was considered to be the eye, was observed a little before the middle line, and near to the superior edge. In the course of a short time, a few began to make some efforts to escape. After they had done so, they were found to resemble, in their external appearance, the young cyclopides alluded to above. At first, the efforts to escape were feeble, but latterly they became more violent; and by means of the tail, which was suddenly and forcibly jerked upwards and downwards, the membranes which contained them were burst on the abdominal surface, upon which the young animal escaped. It was some time, however, before the extremities were completely freed. In the course of ten or fifteen minutes after they had been taken from the body of the mother, these young animals were all free, and the empty sacs were lying amongst them. They have a striking resemblance, in their external appearance, to the larvæ of the cyclops; and if we had not had the certain evidence of having seen them taken from the body of the mother, we would have pronounced them young Cyclopides.

After many fruitless endeavours, we found it impossible to preserve them alive for any length of time, and were, therefore, disappointed in our expectations of seeing them undergo their metamorphoses. We were, therefore, uncertain whether they underwent a first and second metamorphosis, and changed first into the mussel-like form described by Thompson, and then into the parent form, or were simply metamorphosed into the parent form. Seeing that this is a distinct species from that described by Mr Thompson, it is impossible to decide this question until farther observations have been made. Having been fortunate enough, however, in making a series of observations of the same nature on the young of the *Balanus balanoides*, which are recorded above, it will now be seen that this question is already decided, viz. that the balani must undergo two changes of form, or perhaps more, before arriving at a state of maturity.

We will now proceed to give a short description of the larva of this species (Pl. IV., fig. 11).

When viewed from above, the body of the animal is found to be pyriform, with the anterior edge rounded, and the posterior extremity ending by means of a point. The whole body consists of three segments: the first forms the greater part of the body; the two last are minute. Two long unarticulated extremities project from the anterior edge on either side of the mesial line, arising, apparently, from the abdominal surface of the body. Two short antennæ arise also from this edge, immediately on each side of the above described extremities.

The eye is situated a little behind the anterior edge, and in the mesial line of the body.

Two very strong thick legs arise from each side of this first segment of the body. These are bipartite, each division arising from a pedicle common to both, which consists of three segments. The divisions themselves are apparently unarticulated, but are armed with a number of very strong spines.

The second segment of the body is minute. The third and last is also minute and pointed, and is armed with three strong spines, which are bent to one side (the left side), that nearest the right side being the shortest.*

All of these larvæ swim after the manner of the monoculi, by short and sudden jerks. They propel themselves by means of the two pairs of spined extremities. The tail is also in constant motion.

SECTION IV.—ON THE MAIDRE OF THE FISHERMEN.

Hearing our fishermen often speak of "something" which abounded in great quantities in the Firth of Forth during the summer months, which they called *Maidre*, and of which they never could give me a clear description, I determined to examine it for myself.

It was stated to me that this *maidre* was generally found in greatest quantity round the Island of May, *only* during the summer months, and especially during the time of the herring-fishing.

I find, however, that *maidre* must abound during the spring months also, as the stomachs of the herrings caught at present are in most cases filled with it.

In frequent excursions to the Isle of May, during last year, I found that the maidre consisted of one immense continuous body of minute animals.

The animals composing this immense body were those belonging to the Cirripeds, Crustaceans, and Acalepha.

Of these the Crustaceans existed in the greatest numbers, or rather *masses*, for it gives a faint idea to speak of numbers. The Crustacea were Amphipoda and Entomostraca, the former of which were very abundant, but the latter (Entomostraca) formed the greatest proportion of this innumerable body of animals.

The Acalepha also abounded, of which the different species of Beroe were seen in greatest numbers.

I remarked that the masses of *maidre* abounded most at the sheltered sides of the island. On looking into the water, it was found to be quite obscured by the moving masses of Entomostraca, which rendered it impossible to see anything, even a few inches below the surface.

But if, by chance, a clear spot is obtained, so as to allow the observer to get a view of the bottom, immense shoals of coal-fish are seen swimming lazily about and devouring their minute prey in great quantities. Occasionally small shoals of herrings are seen pursuing them with greater agility. It is in the deep caverns, however, in the sides of the island, where the *maidre* is found in greatest abundance; and accordingly, we find that all those animals pursuing them are found there in greater abundance also.

The fishermen, during the earlier periods of the fishery take advantage of this, and, shooting their nets across the mouths of the caves, alarm the herrings in them, either by throwing large stones from their boats or from the tops of the rocks —and in this way sometimes succeed in taking great shots.

These, however, are not the only animals which prey on the immense bodies of *maidre.*

Great numbers of cetacea often frequent the neighbourhood of the island at this time; droves of dolphins and porpoises, swimming about with great activity; and occasionally an immense rorqual may be seen raising his enormous back, at intervals, from the water, and is to be observed coursing round and round the island.

I have examined great numbers of these cetaceous animals (dolphins and porpoises) within the last few years, and never have seen anything resembling the remains of herrings, or fish of any other kind, in the stomach, although the former fish was very abundant at the same time in the Firth. I make no doubt, therefore, that these cetacea only accompany the herring in pursuit of their common food, viz., Entomostraca and Acalephæ.

I have already stated that it was entromostracous animals which formed the great mass of the *maidre.* Among these I obtained a great number of nondescript species, one of which I shall now describe.

On one of my occasional visits to the Isle of May, I observed that at a considerable distance from the island the sea had a slightly red colour, that this became deeper and deeper as we neared the island; and also that the surface of the water presented a very curious appearance, as if a quantity of fine sand were constantly falling on it. I thought at first that this last circumstance proceeded from rain, but presently I found that both phenomena were caused by a great number of small red Entomostracea, which I had never before observed in such abundance. On further observation, I found that it belonged to the genus Cetochilus of M. Rousel de Vauzeme, who has given a detailed description of his species (C. Australis), the only one hitherto known, in the 1st vol. of the *Annalés des Sciences Naturelle*. This author states, that it is found in the Pacific Ocean, and in the middle of the Atlantic Ocean, about 40 degrees south latitude. It forms, he says, very extensive banks, which impart a red colour to the water, and which furnish a plentiful supply of food to the whales frequenting those seas.

Description of Plates.—Plate III.

Fig. 1. Dorsal aspect of the anterior part of the body of the male Balanus. 2. Abdominal do. 3. Abdominal aspect of the first and second segments of do.; *a*, first segment. 4. Third pair of legs. 5. Fourth pair of legs. 6. Fifth pair of legs; *b*, external organs of generation. 7. Parasite. 8. Second stage of the Larvæ of Balanus Balanoides.

Plate IV.

Fig. 10. Male of Balanus Balanoides, abdominal aspect. 11. Larvæ of Balanus Tintinnabulum. 12. Ovum of do. 13. Ov[illegible]f Balanus Balanoides. 14. Natural size. 15. First stage of Larva of Balanus Balanoides. 16. Natural size. 17. Larvæ of Pedunculated Cirriped, drawn after a figure of Thompson's in the Philosophical Transactions for 1835. 18. Natural size of the male Balanus.

(*To be concluded in our next Number.*)

On a new kind of Phosphorescence observed among certain Annelides and Ophiuridæ. *By* M. A. De Quatrefages.

The phenomena of phosphorescence presented by living beings, so worthy of attracting the attention of physiologists, as well as natural philosophers, have yet been studied only in an incomplete manner. This neglect is no doubt owing to the difficulty of the subject itself. In fact, most of the animals in which this remarkable property manifests itself with the greatest intensity, generally inhabit the sea. The few opportunities for examining them that occur on the shores of the ocean or of the Mediterranean, and the extreme difficulty of conveying to the localities instruments of such delicacy as a careful examination of these phenomena requires, have hitherto prevented this subject from being treated with the care which it deserves.

Notwithstanding the small number of facts and observations which we possess in reference to it, we believe we may already conclude, that under this common appellation of phosphorescence, we are in the habit of uniting many phenomena which possess very distinct natures. We appear to confound the property which is inherent in some bodies, of disengaging light during their decomposition, with the analogous phenomena which living beings present. Among these, we have not hitherto established any distinction. It appears to us more than probable that the phosphorescence of decaying wood and fishes, &c., is due to a slow combustion. We are also much disposed to attribute to the same cause the light given off by some animals, particularly that disengaged by the Lampyres and the Elateridæ. This explanation, however, becomes doubtful when we apply it to the animal secretions of certain mollusca. We have, at all events, heard M. Milne Edwards, in his lectures, cite a fact on this point which is highly curious. Wishing to place some living Pholades in alcohol, he observed a luminous matter exude from the bodies of these mollusca, which, on account of its weight, sank in the liquid, covering the bottom of the vessel, and there forming a deposit as shining as when it was in contact with the air.

During the sojourns I have made upon the shores of the British Channel, I have often had occasion to observe the phosphorescence of the sea. So far as my observation has extended in deep water, however, the phenomenon is always limited to sparks, bright but not numerous, which the dash of the oar or the prow of the vessel instantaneously produced, and which as rapidly disappeared. In other circumstanc[illegible]ain, I have noticed the fuci which grow upon the beach resemble an entirely incandescent mass. To witness this spectacle, which I observed particularly at Chausey in the summer of 1841, all that was necessary was strongly to agitate some of the branches which had recently been left bare by the receding tide, when my hand became immediately as if set on fire. It was principally annelides which produced this light; and scarcely less so small ophyures. Besides, I am also sure that some microscopic entomostraca also become momentarily luminous; and it is to these last, especially, that I attribute the very brilliant scintillations I have witnessed on several parts of the coast. At all events, in examining with my glass, with the greatest care, a certain quantity of water taken from these localities, and which gave out sparks in my flask, I have found no other creatures but these last-named animals.

The observations which follow refer solely to the annelides and the ophiuridæ. The former consisted of some minute species of nereidæ, especially of the genera Syllis and Polynœ, some of which I regard as new, but will not here describe. We as yet know too little of these insignificant creatures which swarm on the margin of the ocean to enable us to recognise, with any degree of certainty, whether they are full-grown or otherwise, except when we find them bearing eggs; and, under the circumstances, I have thought it better to abstain from conjecture. At the same time, one of them, which most strikingly exhibited the phenomena of phosphorescence, presented a zoological character which is quite remarkable—namely, that the aciculi with which its feet are armed, instead of being of a conical form as is usually observed, terminate in a kind of pallet with cutting edges, divided into two not very distinct lobes, the one of which is more developed than the other; a mechanism very favourable for wounding any of the enemies of this minute annelide.

One of these annelides found at Chausey, in the testa of a balanus, was nearly twelve lines long, and scarcely a quarter of a line in diameter. Its feet were very numerous and approximate. The moment that I accidentally touched it, it commenced to creep rapidly, and at the same time became so luminous, that I observed this luminosity in spite of the glare shed by a lamp burning with a white flame. This luminosity exhibited a well-marked and beautiful greenish hue. On placing the animal in the dark, it appeared to be luminous throughout its whole extent; but upon examining it with a glass which magnified five or six diameters, I immediately discovered that the light arrayed itself in points which formed two parallel lines throughout the length of the body, and corresponded to the feet of the annelide. These luminous points were very brilliant, and seemed to move about; they vanished when the creature was at rest, and reappeared so soon as a slight excitement induced it to move again.

Wishing to discover the phosphorescent organs with greater precision, I placed the annelide under my compressor. In [illegible] operation it was divided; but both fragments remained luminous. I first employed a lens which magnified only 10–15 diameters, and took care so to arrange the light, that it was so obscure that I could at once distinguish the light produced by the annelide, and the spot whence it issued. At the first motion which the animal made, I now saw each of the luminous points I had formerly observed display itself under the form of a star, whose body was formed by the base of the fleshy organ whence the slender bristles spring. The rays were prolonged along the muscles, which proceeded in all directions from this organ, like the cordage of a ship, towards the different points of the ring. I had abundant opportunity of examining it with all possible care, and I could not discover the slightest trace of phosphorescence in any other part of the animal.

When I employed a lens which magnified thirty diameters, I had great difficulty in discovering the sparks, their brightness being so much diminished. I did, however, succeed; and, by varying the amount of illumination—sometimes dispensing with it, and again employing it—I became quite assured that the light manifested itself only in those muscles which were in action, and solely at the instant of their contraction. Moreover, it scarcely ever extended throughout their whole length, and appeared sometimes only towards their centre, and at other times towards their points of attachment. It was always strongest in proportion as the contractions were energetic. Finally, this light did not appear in a uniform manner, but as if composed of a great number of minute sparks; and the impression it produced on my eye was in every respect similar, though more feeble, to that which results from an electrical discharge occurring through the medium of a great number of small metallic plates placed in juxtaposition.

Under these different manipulations, my annelide was broken into a great number of fragments. All of them, however, more or less maintained their luminous properties; and all that was required to reproduce the phosphorescence was to move them with a pin. With them, as with the halves which at first were the objects of my examination, muscular contraction and the appearance of light were phenomena which appeared thoroughly connected with one another.

After more than an hour's observation, I compressed the remaining fragments between the plates of my instrument, so as to crush them. During this operation, there was not the slightest trace of light; but when I relaxed the amount of the pressure, and at the instant that the two glasses rapidly separated by the action of the spring, it afresh presented itself. This phenomenon was noticed several times, and under sufficiently high powers. I still recognised that the parts which displayed it were always and solely the muscular organs, which, lengthened and flattened by the pressure, regained their natural state upon its removal.

The observations mentioned above were made at Chausey in the year 1841. Occupied with other pursuits, I could not at that time follow them up; I have since, however, completed them, at St Waast-la-Hougue, during the year 1842.

The annelides of this locality exhibited nothing that was new to me; but I noticed, in some of the microscopic species, the facts I am about to detail. I ought here to remark, in general, that the phenomena did not on this occasion exhibit themselves so conspicuously; but whether this was owing to the character of the season, or to the phosphorescence being weak in the species I captured, I cannot say. Nevertheless, I did very clearly observe them in a small polynœ 3–4 lines in length. With the naked eye, I could recognise that the creature was not luminous throughout its whole extent, and that the phosphorescence was confined to certain points upon the sides of the body. Under a magnifying power of 3 diameters, I perceived, as I had done before, every one of these points decompose itself into the appearance of a star, and upon the muscles I could distinguish some of the isolated scintillations, which together formed the radiation of the star.

The minute greyish ophiura displayed appearances altogether analogous. Often, the instant they were touched, they threw their five arms into action, and sparkled from one extremity to the other. The colour they exhibited was of a yellowish green. Their body remained quite obscure. With the naked eye, it clearly appeared that the light upon the arms was not uniform, but that it issued forth from those spots which corresponded with the joints. In watching them with a suitable microscope, and with the necessary precautions, it was apparent that those points were composed—not of a star as in the annelides, but of parallel luminous striæ. Altogether they formed a kind of phosphorescent ring round the joint.

From the facts we have narrated, it follows, that in certain annelides, the motor muscles of the feet are the sole seat of the phosphorescence. The light which issues from these parts is confined to a completely shut cavity, surrounded with a liquid, in which are completely enveloped the parts endowed with functions perfectly determined, and in which there is no room to suspect the presence of any secretory organ whatever. Its appearance always coincides with the contraction of the part, and disappears when this does. From these facts we think we may conclude, that in these animals there is the production of light in the form of sparks, independent of all secretion of other matter. This phenomenon, therefore, in no respect resembles what is observed in those insects which are possessed of a distinct luminous organ, which appears almost formed of a net-work of minute air-cells,—in which the light is remarkably enduring,—in which the phosphorescent matter may be collected after the animal's death, maintaining its peculiar properties after it has been isolated. No more does the phosphorescence of the annelides resemble that of the Pholades, or of the Medusæ, &c., since, in these animals, it depends upon the presence of a mucus which can be collected in considerable quantities.

These observations respecting the annelides are equally applicable to the ophiuridæ. In fact the arms of these latter are composed of minute calcareous portions, articulated severally to the extremities of each other, as in the vertebræ of the tail of the lizard. The living matter with which they are covered is not merely a homogeneous substance—a kind of animal pulp, as some authors have conceived: different tegumentary layers are readily distinguished in it, and the more solid portions are conjoined by true muscular fasciculæ, whose fibres may be perceived by the microscope. It is upon these points alone that the phosphorescence appears: there it exhibits itself under the form of scintillations,—the striæ which it appears to form having the same direction with the fibres; and it does not manifest itself, except when the arm is in motion: As soon as the animal is in a state of repose, no trace of it can be seen, even although the same portions be irritated. Hence, we conceive we may conclude, that in them, as in the annelides, the light is produced in muscular parts, only during their contraction, irrespective of the contact of air, and independent of all secretion, properly so called.

The manner in which the light is distributed upon the muscles of the annelides appears worthy of remark. I have stated above that it seldom occupies the whole of their extent, but appears only, sometimes at the extremities—sometimes in the middle—and sometimes at isolated points. These facts appear completely to agree with what I have elsewhere stated concerning the contraction of the muscular fibres, which I have observed in the Edwardsia and the Synaptes. It may be remembered by some, that I have elsewhere demonstrated that the light scarcely ever occupies the whole length of one of these fibres; and in their case, I can readily judge, by the minute transverse striæ which exist.* In the annelides, again, in which each muscle is formed by a single thread of muscular substance, which is often sufficiently irregular, and in which, during the contraction, only a simple movement of retraction can be perceived, it was impossible to ascertain whether the movement occurred in the whole muscle, or only in a part of it. But a contraction in the phosphorescent species being accompanied with light, the verification of this fact became easy, and I believe I may safely conclude, from the facts above enumerated, that in the annelides, and also the actiniidae and holothuriae just named, muscular contraction but rarely takes place in the whole length of a muscle, or even of a single fibre. Observations of this kind are made with more difficulty among the ophiuridae, in which the muscular fibres are merged into each other, and lost as it were in the general meshwork of the tissues; but it is evident that analogy would authorize the admission of precisely the same conclusion in them, and even in those cases where we have the appearance of isolated luminous points, as I have frequently witnessed, and which at first does not appear confirmatory of the view.

The annelides subjected to experiment have exhibited another fact which appears equally worthy of attention. At the time I discovered them in the fuci, which supplied them with a safe retreat, their movements were prompt and energetic; and the phosphorescence was equally lively. Soon, however, they became languid, as regarded their locomotive powers, and the light became proportionably fainter. After a time, it became necessary to excite them greatly, before a spark of any brilliancy was obtained. At last they could not be roused at all, and the light entirely disappeared. After leaving them, however, to repose for a longer or shorter time, they seemed to renew their strength, and movement returned, together with the luminous phenomena. This kind of lassitude, which was rather tardy in its manifestation in some of the annelides, on the contrary exhibited itself very promptly among the ophiuridae. In these the phosphorescence was at first very conspicuous, and their five arms were often illuminated from one extremity to another; but this brilliancy continued only for a few seconds, and the animal was quiet. Being stimulated, it again put itself in motion, but with much less energy; and the illumination disappeared equally rapidly. Usually, I could obtain only seven or eight consecutive luminous discharges: some of the most robust have afforded as many as nine. One of these, after I had allowed it to rest for half an hour, gave, at the end of this time, three other discharges, but they were far from being strong.

It thus appears very clearly, that the production of this luminosity very much fatigues the animal, and rapidly exhausts it. Where any ophiuridæ were in this condition, I have pricked them thoroughly, and tortured them in all ways, during which they remained quite motionless. If, after this, I, as it were, triturated one of their arms with a pin, I obtained a little feeble light, but nothing more. Even organic contractility appeared to become extinct in them, whilst in one of the genus Syllis, we have previously seen how very long it continued. This marked difference appeared to depend upon the nature of the tissues, which are much more solid in the Articulata, even those which are microscopic, than in the Radiata. The organized layer which clothes the testa of the ophiuridæ, is very rapidly decomposed and dissolved; and it cannot long preserve those properties which appear to appertain exclusively to the organization.

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* See the Memoirs upon the Synaptes of Duvernoy, and on the Edwardsias, as well as the plates which accompany them, in the *Annales des Sciences Naturelles* for 1842.

These facts which I now publish, appear clearly to indicate a resemblance between the luminous phenomena of these minute animals and the electrical phenomena which are possessed by the torpedo, gymnotus, &c. In both of these classes, the imponderable fluid, light or electricity, is wasted by an organized apparatus in a state of purity. In the fishes, as in the annelides and ophiuridæ, this fluid is smartly disengaged by discharges: these discharges, electrical or luminous, progressively diminish in intensity; they speedily fatigue the animal submitted to the experiment, and a longer or shorter repose is necessary ere the phenomena can again be produced.

M. de Humboldt has kindly communicated to me certain unpublished observations made by M. Ehrenberg, and which corroborate those above detailed. The illustrious microscopist of Berlin has observed among some small marine animals approximating to the *Noctiluques* a special luminous organ which he compares to the apparatus of the electrical fishes. He has also been led to admit that in this organ there is the appearance of light by sparks, and by a discharge, and independent of all material secretion. In short, he has, as I have done, assimilated the production of light in these microscopic beings, to the production of electricity in fishes.

There is, however, a fundamental difference between the facts I have detailed, and those observed by M. Ehrenberg. In the annelides and ophiuridæ no special luminous organ is to be found; and the function is devolved upon the muscles. This last particular, however, is not of a nature to prejudice the minds of naturalists, since the beautiful experiments of M. Matteucci have, in the higher animals, demonstrated that the existence of electric currents are manifestly apparent in the muscular masses. Natural philosophers are coming every day to consider the different imponderable fluids as being only simple modifications of one and the same agent; and it is no difficult matter to conceive that this agent, when put in motion in living bodies, and particularly in the muscles, may exhibit itself sometimes in the form of light, and at other times under that of electricity.

I shall conclude this notice with this single reflection. It is well known that in the electric discharges of fishes, the shock, though very strong, is accompanied with only a feeble and dull spark. The ingenious experiments devised by M. Masson, have, it is true, demonstrated, that with a very feeble source of electricity we may obtain very violent shocks, in which notwithstanding, the sparks are scarcely visible. With the help of his apparatus, he has produced the electrical phenomena of fishes in all their minute circumstances, but, in the explanation which he has given of these facts, he is always obliged to admit, that at the moment of the shock, the electricity accumulated in the spinal nerves rapidly; that it consequently acts in considerable quantities, although the source whence it emanates appears to give origin only to a very feeble current. In the theory of M. Masson, or in that of others, the violent shock which is felt in coming into contact with the torpedo and the gymnotus, is owing to a great mass of electricity dispersing itself rapidly among our organs, and the disproportion between the intensity of the luminous phenomena observed, and that which we should have been led to expect, does not the less exist.

But in the annelides and the ophiuridæ we have been able to recognise only the production of light: it would be interesting to discover if there be not at the same time traces of electricity. This investigation cannot evidently be undertaken upon the species which we have observed, their minuteness opposing all attempts of this kind. But it is well known that M. Dugès has discovered in the stones in the neighbourhood of the volcano of Agde a large specimen of the genus Syllis which he has designated by the specific appellation of *fulgurans*, whose luminosity is without doubt of the same nature as in its congeners. It is probable that this species is not confined to the locality in which the Montpellier Professor has discovered it; and I shall take the liberty therefore of pointing out this animal to those naturalists and physiologists who are on the shores of the Mediterranean as a most suitable subject for the experimental enquiry to which I have alluded. It would be curious to discover in the modifications, luminous or electrical, of the agent on which we have been dwelling, a kind of balancing or fluctuation, whence it should result that in the same animal, the one cannot, so to speak, predominate, except at the expense of the other.*

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* I must here make one remark concerning the facts I have been detailing. We cannot on all occasions procure animals whose phosphorescence is so strong that it will be seen when we use magnifiers. Generally we only discover a feeble glimmer when using a magnifying power of ten diameters. Naturalists therefore who wish to repeat these observations must not be discouraged if they meet with some disappointments before they succeed.—*(From Annales des Sciences Naturelles, Mars* 1843, p. 183.)

On some Experiments made by a Commission of the Royal Institute of the Pays Bas, with a view of verifying the property ascribed to Oil of calming the waves of the Sea.

The Annales de Chimie et de Physique for the month of March 1842, contain a Memoir by M. A. Van Beek, on the property possessed by oils of calming waves, and rendering the surface of the water perfectly transparent. After citing many testimonies to prove the existence of this property and its efficacy, the author goes on to express the opinion that we may find, by the use of oil during tempests, a means of protecting piers and other marine constructions against the violence of the waves, by pouring it on the water not far from the sides.

So bold and singular a supposition could not fail to attract the attention of men of science; accordingly, the Academy of Sciences of Paris appointed a commission to examine the subject. But, on this occasion, it will be neither useless nor uninteresting to our readers to know that the same question has already been agitated in Holland.

M. Van Beek, who is a member of the Royal Institute of the Pays Bas, made a proposal last year at the sitting of the class of sciences, having for its object to prevail on the Government to institute experiments with the view of proving that oil had the power of preserving piers against the violence of the sea.

This proposition was not generally approved of. Three members were chosen to examine further into its importance; but these three persons in their turn being by no means unanimous in the considerations and advice which they offered, it was thought that the best way of getting rid of the embarrassment, was to adjourn the consideration of the proposal, and endeavour, before resuming it, to obtain some positive light on the question itself. In consequence, a commission of five members was appointed, with instructions to make direct experiments on the power oil exercises on the waves near the coast; and it is the report of this commission which we are now about to communicate.

The commission nominated from among the members of the first class of the Royal Institute of the Pays Bas, and directed to make experiments on the power attributed to oil and other fat substances of diminishing the violence of waves, report what was done and observed by them on this subject.

The Commission having chosen the village of Zandvoort, situate on the shore of the North Sea, as the place for making their experiments, agreed to meet there on the first stormy day.

They were obliged, however, to change their intention, and to fix on a certain day, on account of the period of the season (the month of June), during which tempests are rare; and the blasts of wind of any degree of strength being also of short duration, it would have been impossible for them to have met at the village mentioned in proper time. They came more readily to a decision by considering that, if oil really exercises on the water in a state of great agitation the power supposed, it must be still more easy to recognise this property on a sea put in motion by a wind of moderate force. Meanwhile, two of the commissioners, happening to be in the country on a day when the wind was blowing violently, made a trial by pouring a small quantity of oil on the water of a rivulet, and observed an evident change in the appearance and movement of the water.

Another member of the Commission made on the same day a similar trial on the Spaarne (a small river near Harlem), and obtained the same result.

Encouraged by all these observations, the 28th of June was fixed on for the purpose of proceeding to ulterior experiments.

The Commissioners assembled at Zandvoort on the day mentioned at nine o'clock in the morning. Some of them proceeded a short distance from the shore, in order to pour the oil upon the water, and observe the results; the others remaining on land, and not knowing either at what moment or how many times the oil was poured out, were to keep their eyes fixed on the waves, which rolled from the boat towards the shore; by these means, their opinion, exempt from all influence, might be considered as so much the more impartial.

The wind was south-west, and of moderate force; the quantity of oil poured out at four different times, namely, at 43, 45, 50, and 54 minutes past nine o'clock, amounted to 15 litres (upwards of 3 imperial gallons); the tide was flowing, and would not reach its full height till 21 minutes past eleven o'clock.

The Commissioners who remained on the shore not having remarked any effect which could be ascribed to the effusion of the oil, and the same thing being the case with those engaged in pouring it, we might already consider the question, if oil poured at a little distance from our piers could protect them from the fury of the waves, as answered in the negative. Nevertheless, the Commissioners thought it incumbent upon them to make a second trial at a somewhat greater distance from the shore. Two of them were rowed beyond the rocks, and then cast anchor.

The distance was calculated by the boatmen at 300 yards; the sounding line indicated a depth of about three yards; and the waves were rolling considerably. More than the half of 15 litres of oil was poured out in the space of five minutes (from 15 to 10 minutes before 12 o'clock), and the Commissioners did not observe the slightest effect in relation to the object of their mission. They saw the oil swimming on the surface of the water, partly united in spots of an irregular form, partly extended and forming a pellicle, and partly mingling with the foam of the waves, and sharing in their oscillatory movements.

When returning to the shore, at the moment of passing the rocks, the Commissioners caused the rest of the oil to be poured on the water, and they can testify that it had no effect in diminishing the motion of the waves, for they were many times abundantly sprinkled with the spray. It is unnecessary to add, that those who remained on land, had remarked nothing at all which could be attributed to the effusion of the oil.

After all that has been said and written on this subject, the Commissioners are astonished at the negative result of their experiments, and, limiting themselves to the account of them, they add no observations. They believe themselves, however, authorized to assert, as their personal opinion, that the idea of protecting our piers by means of oil, is not a happy one.*

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* From Annales de Chimie et Physique. T. vii. p. 371.—The experiments appear to have been conducted on too small a scale to afford satisfactory results. —Edit.

1. *The Permian System of Rocks.* 2. *Theory of the Origin of Coal.* 3. *Lines of Ancient Sea Levels.* 4. *On Mastodontoid and Megatherioid Animals.* 5. *The Chief Aim of the Geological Society of London.* By R. I. Murchison, Esq., President of the Geological Society, &c. &c.*

1. *Permian System of Rocks.*—On its eastern frontier, far removed from the tract to which allusion has been made, the uppermost member of the carboniferous limestones of Northern and Central Russia, distinguished by the presence of multitudes of the foraminifer *Fusulina*, is succeeded by the most widely spread of the Russian systems; to which, from its occupying the whole of the ancient kingdom of Permia, we have assigned the name of Permian. You have been told, that this vast group is composed of limestones, marls, great masses of gypsum, rock-salt and repeated alternations of cupriferous strata; and that it contains a flora and a fauna, of characters intermediate between those of the Carboniferous and Triassic periods. The shells are, to a great extent, those of our Magnesian Limestone or Zechstein; and, like the conglomerate of that deposit near Bristol, the Permian rocks are distinguished by the presence of Thecodont Saurians. The interest attached to these vast deposits, which have been spread out on the western flanks of the Ural Mountains, is increased by the inferences which have been drawn, that springs and currents holding much copper in solution must have flowed from the edges of that highly mineralized and metamorphic chain, while the Permian strata were accumulating. But the great value of having worked out a fuller and richer type of a group of strata between the Carboniferous and Triassic epochs than any which exists in Western Europe, will be found in the fossil shells, the plates of which are already far advanced; for, with some species hitherto known in the Zechstein

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* From the Address delivered at the Anniversary Meeting of the Geological Society of London, 17th February 1843.

of Germany and Magnesian Limestone of England, we shall publish others which are indentical with or analogous to forms that occur in rocks occupying the same geological position in North America.

In America, indeed, as in Russia, these beds had been compared with every deposit, from the coal to the Keuper inclusive, whilst in our work they will be shewn to have no connection with the New Red Sandstone or Triassic group, but to occupy a definite position, truly intermediate between that system and the carboniferous. At the same time it is manifest, that although they overlie and are, as they ought to be, very distinct from the Carboniferous system, yet they contain some species of shells which occur in that division. Thus it will be made evident, that after all there now remains scarcely any real difference of opinion on this head between Mr Phillips and myself (to which I alluded last year); for I learn from him, that in England the analogy between the fossils of the Magnesian and Mountain Limestone obtains to a far greater extent than could be supposed from any published catalogues. I trust, therefore, that the ensuing year will not be without its fruits in the production of new works on the shells of the Magnesian Limestone of our own country; and I am glad to have it in my power to inform you, that Mr King, the Curator of the Natural History Society of Newcastle-on-Tyne, is preparing some excellent materials for this purpose.

A better acquaintance with the Permian fossils, particularly the prevalent Mollusca, induces me, notwithstanding the arguments I employed last year, to infer that this deposit, so naturally connected through its characteristic fossils with the Carboniferous strata, must be classed with the Palæozoic rocks.* The physical structure of Russia is also greatly in favour of this view; for, in large portions of that country, there is an entire absence of the great rupture between the Carboniferous rocks and the Magnesian Limestone, which is so prevalent in the British Isles. The examination of rocks of this age in North America, leads to the same opinion; viz. that the Permian deposits must be viewed as the fourth or uppermost stage of the Palæozoic series, notwithstanding the occurrence of Thecodont Saurians.

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* My companions, M. de Verneuil and Count Keyserling, have long entertained the same views as Mr Phillips on this point.

2. *Theory of the origin of Coal. American and European Evidences compared.*—At the last Anniversary we were aware, from the independent evidence of Mr Lyell, that both the bituminous and anthracitic coals of Pennsylvania were underlaid by *Stigmaria ficoides* and fire-clay; and we have now before us the result of the labours of our associate Mr Logan in the coal-fields of Pennsylvania and Nova Scotia, in examining which, his chief object seems to have been to ascertain whether the facts relating to the theory of the origin of coal, as seen in North America, were analogous to those to which he has so successfully directed attention in England.

Availing himself of the prior researches of the American geologist, Professor H. Rogers, and his assistant surveyors, who had prepared the valuable map of Pennsylvania above alluded to, Mr Logan has laid before us a very clear sketch of the general relations of the Pennsylvanian carbonaceous deposits, and of their chief convolutions. Since that time the Governor and legislature of the Canadas have wisely selected this well-trained field-geologist to execute a mineral survey of the whole province; and I am happy to acquaint you that he has already commenced his task in a very effective and vigorous manner, by laying down as the base-lines of his work, some of the great anticlinals and synclinals of that region, and by connecting them with the already described features of the United States. In comparing the coal-field of Pennsylvania with those of South Wales, with which he is familiar, Mr Logan states, that he almost invariably detected beneath each anthracitic coal-seam, a bed of fire-clay or argillaceous materials filled with *Stigmaria ficoides*. In his description of the coal-fields of Nova Scotia, which have not yet been fully developed, but among which we hear of one bed of clear coal twenty-four feet thick, and affording 250 tons daily, Mr Logan states he had also detected the *Stigmaria ficoides* in similar underclay. With such extended observation spread out before them, the evidences in which all seem to point one way, young geologists may well be led to suppose that the theory which, if I may so speak, has recently been rendered fashionable, of the origin of coal by subsidence of vegetable matter *in situ*, must be considered established as of general application. I, however, adhere to the cautionary remarks which I ventured to make last year, and will now endeavour to impress upon your minds the inapplicability of such a theory, however true under limitations, to large portions of the carboniferous strata in different parts of the world.

Since our last Anniversary statements have appeared in our own country, both supporting and impugning the probable truth of the theory. The last meeting of the British Association being held at Manchester, geologists were there assembled in the centre of a tract appealed to with great reason by the supporters of this theory as containing many proofs of its truth; for, in the immediate vicinity of that town there occur, as you all know, the beautiful examples of vertical stems of large trees apparently in their original position, which were formerly described before this Society. After giving an elaborate and satisfactory account of the great Lancashire coal-field, shewing that its lowest members, formed on the flanks of the Penine chain, and subordinate to the millstone grit, contain marine shells analogous to those of the Mountain Limestone series, and stating that they are surmounted by a middle and an upper group, the former constituting the richest coal-field, Mr Binney describes in great detail the composition and contents of all the numerous roofs and floors, as well as also of the coal-seams, which are included between them. He shews also that the roofs vary in their nature at different places, even over the same seam, and contain the remains of many vegetables, sometimes, as near Manchester, in vertical positions, *Sigillariæ* being in such cases a most abundant plant; other roofs of black shale in the lower field are loaded with *Pectens*, *Goniatites*, *Posidonia*, and fishes. The coal-floors, on the contrary, present a much greater uniformity of structure, fire-clay similar to the underclay of Mr Logan being most abundant; though it is admitted, that a different or siliceous clay also frequently occurs, and that two instances are known where the coal rests at once on coarse quartzose sandstone. Seeing, that with one exception, all the floors throughout an estimated thickness of near

5000 feet contain the plant *Stigmaria ficoides* usually with its leaves attached,—that both the roofs and floors indicate a very tranquil method of accumulation,—that the coal is free from admixture of foreign or drifted materials, and that large trees frequently stand upright, this author is induced to believe that the vegetables out of which the coal has been formed, grew upon the spot.

At the same meeting, this view was contested by Mr W. C Williamson, also well acquainted with the structure of the country around Manchester. His chief arguments were, however, derived from other tracts, and they assisted in proving,—1*st*, The frequent association of marine shells with coal (as at Coalbrook Dale, and in Yorkshire). 2*dly*, The very triturated and broken condition of the plants, as well as their great intermixture in the sandstone and grits, coupled with the fact that large quantities of vegetables are often matted together with marine and estuary shells, phenomena indicative of drift. Admitting that the floors of the coal or underclay present a great uniformity both in the absence of other plants, and in the almost general occurrence of the Stigmaria, Mr Williamson allows that a plant, found so very generally in such a position, may have grown in estuaries into which the other vegetables were drifted. Acknowledging that the drift theory is open to some objections, he stated that one of the greatest of these is, in his opinion, the extent and uniformity of some of the thin seams of coal. On this point, however, I must be permitted to say, that, if admitted, the difficulty must be applied to numberless other deposits of all ages, which every one knows must have been accumulated under water. Subaqueous action of a tranquil nature is, it appears to me, precisely the agency by which we can satisfactorily explain the uniformity of many thin layers containing vegetables which are extended over wide areas, as in the copper grits of Russia before alluded to. By what other possible means, for example, can we explain the wide extent of the thin copper slate of Germany with its associated fishes on the still thinner bone-bed at the base of the Lias? So far then from being a phenomenon which invalidates the formation of coal under water, it seems to me, that the very fact of a thin and equable deposit is an almost impossible condition, if we insist exclusively upon the submergence of forests or jungles *in situ*, in which considerable irregularities of outline must in all probability have prevailed.

On my own part, and that of my fellow-travellers in Russia, I have brought before this Society what we consider strong evidences against the too general adoption of this favourite theory. We have told you that in many instances the *Stigmaria ficoides* occurs in loose and incoherent sands, as well as in shales, and is frequently present where no coal is seen; but what we chiefly insist upon is, that all the coal-seams of the south of Russia, without exception, alternate repeatedly with beds of purely marine origin. In one section of the Donetz coal-field it has been stated, that at least twelve beds of marine limestone alternate in one vertical section with thirteen seams of coal and numerous bands of sandstone and shale, in which many species of plants, besides Stigmariæ, are confusedly heaped together. But we need not go to Russia for such examples. The whole of the mountain limestone or lower coal series of the north of England is charged, though not to so great an extent, with proofs of the alternation of marine deposits with coal and its associated sandstone and shale.

The coast of Northumberland, to the north of Alnwick, presents evidences of thin seams of coal resting at once on sandstone, and intimately connected with limestone full of sea-shells. Advancing northwards to Berwick, and to beyond the Tweed, purely marine strata re-occur, charged with still more carbonaceous matter; and, in the same series on the north-western parts of England, we have frequent examples of the persistence of what must be called exclusively marine conditions. Throughout that vast succession of beds, all the animal remains with which geologists have become acquainted, occupying many distinct stages, have lived in the sea, whilst the plants, so far as I have been able to observe them (broken into fragments), consist of many species irregularly heaped together, the whole, together with the sands, grits, pebbles, and shale, offering the clearest signs of the drifting action of water.

On the subject, then, of the origin of coal, it would appear, that as our inductions can never be sound, if they repose upon one class of phenomena only, so do some coal strata offer indications of the truth of the hypothesis, that in large tracts of the world, the mineral was formed from vegetables which were washed into bays and estuaries, and often carried far into the then existing seas. In other instances, flat and marshy tracts rich in tropical vegetation, being subjected to gradual depressions, may have been converted into lagoons and swamps without any direct encroachment of the sea; and in this peculiar condition (subjected, however, in all cases, to entombment beneath those waters in which the overlying sandstone and shales were accumulated), oscillations of the land may have raised the beds at intervals, again to be fitted for the growth of marshy vegetables.

In geology more than any other science, it must be our constant endeavour to unravel phenomena which at one time seemed inexplicable, and often opposed to each other; but with new discoveries the difficulties vanish, and the apparently conflicting testimonies are found to be in perfect harmony with the order of changes, which the surface of the globe has undergone. I repeat, therefore, my belief, that, whilst coal may have been formed in many localities by subsidence of vegetables on the spot on which they grew, as first suggested by Brongniart, MacCulloch, and others, its origin unquestionably is also due, and over very large territories, to plants having been washed into estuaries and seas, and there equally spread out in successive layers with sand and mud.

3. *On Lines of Ancient Sea-Levels.*—In a recent report to the French Institute, our foreign Associate, M. Elie de Beaumont, has given the substance of a most important memoir by M. Bravais, "On the Lines of Ancient Sea-level in Finmark." Informing us that this work proceeds from the pen of a naval officer attached to one of those numerous scientific enterprises conducted at the public cost, which do so much honour to the French government, M. de Beaumont embodies the labours of M. Bravais in a lucid analysis of many of the facts relating to the same subject, which have been accumulated in Norway, Sweden, and the British Isles.

Proofs of the elevation of the coasts of Norway have been brought before geologists by Von Buch, Brongniart, and Keilhau, and have recently been extended by M. Eugene Robert to Spitzbergen. Mr Lyell has made the British public familiar with the great oscillations which the land of Sweden has undergone since the existence of the present marine fauna (for Scania has been depressed beneath the Baltic, whilst other parts of Sweden have been raised), and I may be permitted to add, that the extent to which elevations have affected the north-eastern corner of Europe has been recently pointed out in Russia by my companions and myself.

In the prelude to the report on the part which M. Bravais has performed in these labours, we are put in possession of the results of the valuable researches of Professor Keilhau, who, prior to the French expedition, had ascertained the levels of different accumulations, all supposed to be marine, from the sea-shores to altitudes of above 600 feet in the interior of Norway.

The greater number of geologists have for some time believed, that these phenomena could alone be satisfactorily explained by upheaval of the land, and M. Bravais has, by a new method of proof, arrived at the same conclusion.

Passing about a year in the environs of Hammerfest, he observed that terraces of gravel in some spots, and marks of erosion on the face of the cliffs at others, indicated at least two ancient lines of sea-level, which extended from the coast far into the interior along the sides of the sea-loch of Alten fiord. Availing himself of the water-mark left by the line of seaweeds (*Fucus vesiculosus*), and estimating from that horizon an approximate mean level of the tide, he instituted a series of exact measurements of the altitude of both the lower and upper sea beaches, or ancient water-marks upon the rocks, at six different stations between the mouth of the fiord and its southern extremity, a distance of ten to eighteen leagues, and he arrived at the striking result, that the two terraces of Alten fiord, which at first sight, or seen only to a limited extent, seemed to be horizontal and parallel, are, when measured rigorously, found to rise from the levels of 46 and 92 feet (English) above the sea near the mouth of the firth to the heights of 90 feet and 220 feet at its further or inland extremity! In referring you to the Memoir for the ingenious and accurate methods employed by the author to obtain these results, and of which M. de Beaumont has given a very clear account, I will here simply direct your attention to some of the chief geological considerations with which they are involved.

As these lines of deposit rise towards the interior, so as to mark that they coincide nearly with the chief axis of elevation of the Norwegian chain, and as there is a want of parallelism in the two beaches, the relative altitudes of which vary much in short distances, so is it obviously impossible to account for the phenomena by any former condition of the tides; and the hypothesis of salt water lakes is, from the same causes, equally inadmissible.

Submarine currents dependent upon violent elevation of the chain will, the author contends, no better explain the phenomena, because the torrential debacles which would have accompanied such movements would have left confusedly assembled drifts, and not regularly arranged terraces. It therefore seems fair to admit that these are truly ancient sea-beaches. The measurements of M. Bravais shew, in fact, that in proceeding from the coast into the interior, these beds not only rise to higher levels, but that their elevation has been irregular, viz. that whilst the sea-ward inclination of the older or higher of the two terraces, taken from a station at the middle of the fiord, is very moderate, the rise of the same beds from that central point to the southern extremity of the fiord is at a greater angle, and therefore, that there has not only been a much more intense movement of elevation over one portion of this area than another, but that this notabe change of dip indicates the greatest movements at the two extremities, the centre varying slightly from the horizontal. Now from these facts (independent of all the geological evidence) it is argued, as before observed, that no change of level of the sea will account for such an outline.

I will pass over those parts of the report which are connected with pure physics, not only on account of my own incompetency to do justice to them, but because I would suggest, that however certain geological phenomena may be eventually proved to be connected with the question of the density of the earth, it is obvious that unequal simultaneous elevations and depressions over so small an area as Scandinavia can scarcely be due to such a cause. After ably treating this subject, and showing that great terrestrial movements only can be admitted as explanatory of the facts observed, M. de Beaumont refers to the works of British geologists, and suggests, as indeed Mr Phillips has done,* that the parallel roads of Glen Roy may, by accurate measurement, be proved not to be parallel; and he then goes on to shew, that the lines of ancient sea-level of comparatively modern periods, have undergone broad undulations or great ascending and descending movements over extensive areas.

Let us see how these views are strictly applicable to our own country. The occurrence of ancient beaches containing marine shells of existing species at different levels above the sea, has long been observed by geologists in the British Isles. Terraces of gravel have also been noted at various altitudes. In some instances they have been referred to the formation of lakes, but in others they have been compared to sea-shingle; in many cases also they have been merged with diluvial deposits, and latterly an endeavour has been made to explain some of them by the action of glaciers which are supposed to have barred up former lakes. It may be that we should not endeavour to refer the whole of these phenomena to one common origin; though most persons must admit that the mass of recent evidence proves the greater number of the superficial deposits, to which allusion is now made, to have been formed beneath the sea. Wherever, indeed, these accumulations are found to contain marine shells so imbedded in the sand or gravel as to resemble sea-bottoms, and no doubt can remain of their origin, the only question is, why do we find these shells at such different altitudes? why are the same marine remains (appealing to British examples) placed at a height of upwards of 1600 feet in North Wales, and a few feet only above the sea in Devonshire and Cornwall, and at heights varying from 200 to 400 in the central counties? Most geologists have been satisfied to reply (and I am myself of the number), that in these evidences they had distinct proofs, not that the sea had stood at an indefinite number of levels, thereby making a most broken and irregular outline *within the same period* (a supposition apparently absurd), but that the bottom of the sea had undergone irregular oscillations, some points having been raised to much greater heights than others. The examination of the mountains of North Wales and the adjacent plains of Shropshire, for example, could lead to no other inference; there being in tracts absolutely contiguous, a difference of not less than 1000 feet between the level at which the same species of marine shells are now lying; and as no other portion of England or South Wales offers a trace of testacea at the higher of these altitudes, it is demonstrable, that when the sea deposited the shells in Moel Tryfane it did not stand at that height above the whole of England, but that the local appearances resulted simply from unequal elevation of the sea's bottom.

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* See Penny Cyclopædia, *Parallel roads.*

All the analogous phenomena in the British Isles seem to lead to the same conclusion. Whilst the modern marine alluvia of the central counties are found to rise towards Snowdon as a great centre of elevation, the banks of gravel with similar shells ascend from the coasts of Lancashire towards the Penine chain in the interior. Again, in the south-west of England, the most distinct sea-beaches yet noticed, were ascertained to rise very perceptibly from a low level on the south coast of Devon and Cornwall, to heights of 120 feet above the sea in North Devon, where the elevation is more intense. The valley of the Severn afforded similar proofs; the beds of gravel with sea-shells, between Worcester and Gloucester, near its estuary, are slightly elevated above the sea; but in ascending to its source, the same gravel and shells occur at altitudes of 500 to 600 feet, until finally they are seen in the lofty cliffs of Moel Tryfane, before alluded to.

Whilst such may be justly received as absolute proofs (quite as clear as those of M. Bravais) of more intense elevation at some points than at others, the submarine forests along our coasts have been supposed to offer proofs of subsidence. These evidences, however, are not of the same satisfactory nature as those of elevation, for it may in some instances be contended, that the forests in question grew upon low deltas, and have been overwhelmed by irruptions of the sea, which broke down certain banks or natural barriers, that at one period protected them from inundation. But granting these submarine forests on the east coast of England to be really as good proofs of a depression of the land as any which exist on the shores of Scania, where a great subsidence has been established, it must also be borne in mind, that the rocky western coast of our island offers equal signs of depression, since Cornwall and Pembrokeshire, and even Cardigan, so near to the point of intense elevation, Snowdon, have each their submerged forests as well as Lincolnshire. Examined, then, as a whole, England offers many evidences which to me seem conclusive, that within a very recent period, the land has undergone great and unequal movements, both of elevation and depression, in relation to the level of the sea. Scotland and Ireland present like phenomena. In the latter country modern marine shells have been found in many localities at various altitudes. Scotland so rich in superficial accumulations, also offers near her shores many testimonies of former sea-bottoms laden with numerous shells, but hitherto these remains have only been found at comparatively low altitudes. Another class of detritus, in which, in common with England, she is rich, consists of extensive irregular accumulations of clay, boulders and gravel, usually called "till," and which may be compared with the drift of the tract extending from Bedfordshire and the eastern side of Huntingdonshire, to the coasts of Norfolk, Suffolk, and Essex. A third class is composed of gravel and sand, often arranged in terraces, which in some cases occur at different levels, following the sinuosities of the bays and headlands of the sea-coast; in others ramifying into the interior, along the sides of deep cavities occupied by fresh-water lakes.

The well-known "parallel roads" of Lochaber offer the most striking example of terraces at different levels above a series of existing lakes, and their explanation has been long the subject of controversy. For many years it was the favourite hypothesis, based, however, upon the supposition of their *perfect parallelism*, that these lines of shingle were the shore lines of the lakes when they stood at higher levels, from which they have been successively let off by the breaking down or wearing away of their barriers. Though supported by several good observers, this view has always presented great difficulties as to the demand upon our belief in the wearing away and destruction of enormous barriers. Very recently, indeed, the expounders of a terrestrial glacial theory have at once obviated all difficulty, by constructing in their imagination enormous walls of ice 2000 feet high, by which such lakes were formerly supported, and by the sudden melting of which they have been let off.

Unable to screw my courage up to the belief in this glacial explanation, I will not now repeat the many objections which must be raised against it, but will simply join those who prefer to invoke the more rational, and, as it appears to me, perfectly satisfactory hypothesis, that all these terraces of gravel (including those of the parallel roads) are nothing more than ancient lines of beach, which are so many marks of the successive rise of the land. This view is, that, as you all know, which was so ably sustained by Mr C. Darwin, who, in pointing out their analogy to raised beaches in other countries, has also shewn, that as similar materials occur in the great chasm of the Caledonian Canal at still greater altitude (900 feet above the sea), it was impossible to refer them to any other cause than submarine elevation.* I have too long entertained the same opinion respecting most of the great gravel accumulation of our isles, to doubt that this is the true explanation. I would add, however, that the southern shores of the Moray Frith, which are of course open to the wide ocean, offer to my eye still more convincing proofs of the accuracy of this view, than the patches of gravel along the Caledonian Canal. The terraces of gravel, sand, and boulders which there occur at different levels, may, in fact, be traced from the slopes of the mountains of Morayshire and Elgin, to those heaps which lie in the great gorge of the centre of the Highlands ; and therefore I maintain, that all these accumulations must have been once connected, and were all originally formed under the sea.

No one who has read M. de Beaumont's report on the memoir of M. Bravais, can fail to be struck with the strong, nay even direct analogy which the gravel and shingle beds of the marine bays and fresh-water lochs of the Highlands of Scotland bear to the terraces of the Norwegian fiords ; and as the latter author has now by patient observation brought the phenomena observed among the latter under the direct control of geometrical admeasurement, and has shewn that such lines, when correctly examined, are neither horizontal nor parallel, so is it my duty, believing that many of the Scottish gravel terraces have been produced by similar agency, to incite my brother geologists to apply the rigorous method of examination of M. Bravais, and thus to render this branch of our enquiries more exact. I entertain, indeed, the most sanguine hope, that before another anniversary passes over, the Scottish phenomena will be tested in a similar manner with those of Norway; and, that as the beds of marine shells in England and parts of Scotland and Ireland so clearly bespeak great irregularities of movement in the land, so we shall be equally able to shew that in the Highlands, lines of elevation acting from different centres and with different degrees of intensity, have raised former sea-bottoms to the different levels at which we now find them, whether along shores or in the deep lateral depressions by which Scotland is so fissured. Let the memoir of M. Bravais therefore, with the admirable commentary of M. Elie de Beaumont, be the stimulus to those who enter upon this inquiry, which should not be limited to the parallel roads of Glen Roy, but extended to the Western Islands and shores of the lochs of Western Ross, among which I have a recollection of numerous shingle terraces, including that so well described by Captain Vetch*; and eastwards, to the great accumulations adverted to on the southern shores of the Moray Frith, which in their turn should be connected with the elevated shelly beaches of Banffshire, first pointed out by Mr Prestwitch.†

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* Our friend, Mr Maclaren, has supported the same view with his usual ability. —Editor.

In quitting the consideration of this very interesting topic, I cannot however occupy this Chair without saying, that although British geologists have not yet employed the rigorous test applied by M. Bravais, there is no department of our science on which their observations have thrown more light than that which embraces the phenomena of ancient sea-beaches. Reasoning backwards from existing causes and the facts of yesterday, Mr Lyell has, by a well-digested set of observations, led us to contemplate the very period when the ocean was beating against our inland escarpments of chalk, and when our valleys and those of the opposite coasts of France may have been fiords like those of Norway.*

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* Geol. Trans. vol. i. p. 416.

† M. de Beaumont makes full allusion to the British cases.

I also know that my friend Mr Lonsdale has long entertained similar views, derived from his intimate acquaintance with the escarpment of the oolitic strata; along some of which he has observed as perfect lines of dunes as those upon the sea-coast of France, whilst in others he has been struck with their resemblance to many great dislocations of marine undercliffs, whereby masses of the inferior oolite have been pitched into inclined positions at the bottom of the adjacent valleys; and as I know that this subject is one which now occupies his attention, I have strong hopes, that by his present residence on the coast of Devonshire, he will be enabled to add materially to the exact conclusions which have been already drawn in this class of researches.

4. *On Mastodontoid and Megatherioid Animals.*—For a season our metropolis contained within it a magnificent skeleton of a Mastodontoid quadruped, which, in common with all geologists and palæontologists, I hoped to see permanently established in our National Museum. This gigantic animal was discovered by a persevering Prussian collector, M. Koch, who for some time resided in the United States, and who disinterred it, together with a great profusion of heads, teeth, and numerous bones of similar animals, from amid the alluvia of a tributary of the St Louis river, where the chief remains had probably been an object of superstitious tradition on the part of the Indian tribes. It does not appear whether the zealous Prussian had any scruples to overcome; but I presume they must have been considerable, if I were to judge from my own experience in other wild countries. In travelling along the eastern flanks of the Ural Mountains, it was my lot to visit many sites of gold alluvia in which bones of the mammoth and other extinct quadrupeds are found, and for these remains the poor Bashkirs, the original inhabitants of the tract, preserved so deep a veneration, that, in freely permitting the search after the true wealth of

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* Elements of Geology, vol. ii. pp. 3—8.

their country which they were incapable of extracting, their sole appeal to the Russian miners was, "Take from us our gold, but for God's sake leave us our ancestors."

Overcoming, however, all difficulties, M. Koch succeeded in extracting, and afterwards in setting up, the most complete specimen of the species which has ever been seen. Applying to it the provisional name of "Missourium," he exhibited it for some time in the United States, and then brought it with many of the associated bones to London, in the hopes of having the remains perfectly described, and of obtaining for them a price worthy of the British nation.

The arrival of such a collection could not fail to excite the most lively interest and curiosity among our naturalists, and the bones having been attentively examined by many members of this Society, produced a diversity of opinion respecting the generic character of the chief remains. North America had long been a fertile mine of such reliquiæ, and the naturalists of the United States had not been backward in studying and describing them. It is not, therefore, a little remarkable that the same difference of opinion as to the generic and specific identity of the animals that prevailed across the Atlantic, is presented in the memoirs which have recently been read before us; Dr Harlan and Mr Cooper having maintained opinions, with which, to a great extent, Professor Owen concurs, whilst Dr Grant and M. Koch have supported the views of the late Dr Godman.

Citing the American authorities on his side of the question, including Dr Hayes, and enumerating no less than thirteen species of Mastodon and six species of Tetracaulodon, Dr Grant has made a vigorous effort to vindicate the true generic characters of the Tetracaulodon, as founded on the presence of a tusk or tusks in the lower jaw and certain variations in the form of the crowns of the molar teeth.

This view has been sustained by Mr A. Nasmyth in an elaborate paper "On the Minute Structure of the tusks of extinct Mastodontoid animals.'' Microscopical examination of portions of the tusks believed to belong to five distinct species, viz. *Mastodon giganteus, Tetracaulodon Godmani, T. Kochii,* *T. Tapiroides* and the *Missourium*, has also led this author to the same inference as Dr Grant; and he concludes with the remark, that, if it be established that specific differences positively do exist among all these animals, the value of such microscopic researches is great; but if the five animals are grouped as one, then such mode of observation is of no value in palæontological science.

Professor Owen had previously expressed opinions at variance with those of Drs Hayes, Godman, and Grant, and Mr Nasmyth, and his views have been supported within these walls by my predecessor, Dr Buckland. Pointing out certain mistakes in the setting up of the Missourium, as exhibited in the Egyptian Hall, he compares the fossil with all forms with which he was acquainted; and, shewing that it must have belonged to the Ungulata, he judges that the enormous tusks of the upper jaw constitute a member of the proboscidian group of pachyderms, and that the molar teeth prove it to be identical with *Tetracaulodon* or *Mastodon giganteus*. He argues that the genus Tetracaulodon was erroneously founded upon dental appearances in the lower jaw of a very young proboscidian, and that Mr W. Cooper was correct in suggesting that the Tetracaulodon was nothing but the young of the gigantic Mastodon, the tusks of which were lost as the animal advanced in age. A comparison of the whole of M. Koch's collection produced the result in Mr Owen's mind, that, with the exception of a few bones of the *Elephas primigenius* (Mammoth), all the other remains of proboscidian pachyderms in it belong to the *Mastodon giganteus*. The remains of other animals found by M. Koch are referred to by the Hunterian Professor to *Lophiodon*, *Mylodon Harlani*, *Bos*, *Cervus*, &c.; and in respect to the *Mastodon giganteus* he expresses his conviction that it had two lower tusks originally in both sexes, and retained the lower tusk only in the adult male. Although unable to form a correct judgment on the probable structure of those extinct quadrupeds, I may call your attention to a recent work of Mr Kaup, whose striking discovery of the Deinotherium is familiar to you, and who now seems to advocate, from perfectly independent sources of evidence, the same views as Professor Owen concerning the osteology and generic characters of the Mastodon founded upon the comparison of a series of bones and teeth belonging to the *Mastodon longirostris*, more numerous and complete than even those of the *Mastodon giganteus.*

Mylodon.—One of the most brilliant, and, I venture to say, not the least durable of the researches in palæontology, remains to be mentioned in the description of the *Mylodon robustus*, a new species of gigantic edentate animal, accompanied by observations on the affinities and habits of all Megatherioid animals. After a sketch of the labours of Cuvier, who first described the huge Megatherium and pointed out its analogy to the family of Sloths and Armadillos, of the succeeding writings of Jefferson and Harlan upon the genus Megalonyx, of Dr Lund on the Cœlodon and Sphenodon of Brazil, and of his own researches which established the Mylodon and Scelidotherium, Professor Owen proceeds to describe the megatherioid animal which he has named *Mylodon robustus.*

Of the purely anatomical descriptions, it is not my province to speak, and referring you to the work in which, through the enlightened munificence of the College of Surgeons, all the necessary illustrations have appeared, I pass to the generalizations, and learn that the Mylodon, in common with the Megatherium and Megalonyx, are genera of the family of *Gravigrada*, as distinguished from the *Tardigrada* in the order *Bruta.*

Professor Owen then proceeds to a comparison of the anatomy of the Mylodon with that of all analogous creatures, and after an able analysis, he satisfies himself, and also, I am persuaded, every one who has followed his close reasoning, that he has at length ascertained the true habits and food of this family of mammifers. From their dentition, it is inferred that the Megatherium and Mylodon must have been phyllophagous, or leaf-eating animals; whilst, from their short necks, the very opposite extreme to the camelopard, they never could have reached the tops of even the lowest trees. Cuvier, on the contrary, suggested that they were fossorial, or digging animals; and we all recollect the animated manner in which Dr

Buckland attracted us, whilst he described the Megatherium as a huge beast, which, resting upon three legs, employed one of its long fore-hands in grubbing up whole fields of esculent roots; a habit which procured for it the significant popular name of "Old Scratch."

Dr Lund, a Danish naturalist, had considered the Megatherium to be a scansorial or climbing animal; in short a gigantic Sloth. After a multitude of comparisons, Professor Owen rejects the explanation of his predecessors. He shews that the monstrous dimensions of the pelvis and sacrum, and the colossal and heavy hinder legs, could never have been designed, either to support an animal who simply scratched the earth for food, or one which fed by climbing into lofty trees, like the diminutive Sloth; and he further cites the structure of every analogous creature, either of burrowing or climbing habits, to prove, that in all such the hinder legs are comparatively light. What, then, was the method by which these extraordinary monsters obtained their great supplies of food? The osteology of the fore-arm has, it appears, afforded answers which are valuable, chiefly for their negation of erroneous conjectures, such as that the animal was an ant-eater, rather than for the habits which it directly elicits. It is, therefore, to the organization of the hinder limbs that Professor Owen mainly appeals to ascertain the functions of the fore-feet and the general habits of the Mylodon.

Arguing that the enormous pelvis must have been the centre whence muscular masses of unwonted force diverged to act upon the trunk, tail, and hind-legs, the latter, it is supposed, formed with the tail a tripod on which the animal sat. Professor Owen supposes that the animal first cleared away the earth from the roots with its digging instruments, and that then seated on its hinder extremities, which with the tail are conjectured to have formed a tripod, and aided by the extraordinary long heel as with a lever, it grasped the trunk of the tree with its forelegs. Heaving to and fro the stateliest trees of primeval forests and wrenching them from their hold, he at length prostrated them by his side, and then regaled himself for several days on their choicest leaves and branches, which till then had been far beyond reach. After shewing that from the natural inversion of the hind-feet the Mylodon approached to the scansorial animals, and thence inferring that it might have had climbing powers necessarily much limited by the other parts of its frame, Professor Owen states that the inversion of the soles of the feet is least conspicuous in the Megatherium, whose bulk and strength would be adequate to the prostration of trees too large for the efforts of the Mylodon, Megalonyx, and Scelidotherium. The Megatherium, in short, was the mighty tree-drawer, and had therefore no need of the adventitious aid of any climbing apparatus. Allow me to add, that, amongst other reasonings, those which lead to conclusions that one class of megatherioid animals was furnished with a hairy coating (like the Mylodon), whilst another, like the great Megatherium, was devoid of it, as evidenced by slight modifications of the bony structure of the hind-feet, appear to me to be not the least original and interesting.

Wholly incapable, as I am, to do justice to this masterly inquiry by the necessarily brief allusion which is imposed upon me by the nature of this discourse, I shall best execute my task in quoting the words with which Professor Owen sums up his reasoning.

"On the Newtonian rule, therefore, this theory has the best claim to acceptance; it is, moreover, strictly in accordance with, as it has been suggested by, the ascertained anatomy of the very remarkable extinct animals, whose business in a former world it professes to explain. And the results of the foregoing examination, comparisons and reasonings on the fossils proposed to be described, may be summed up as follows. All the characteristics which exist in the skeleton of the Mylodon and Megatherium, conduce and concur to the production of the forces requisite for uprooting and prostrating trees; of which characteristics, if *any one were wanting, the effect could not be produced*: this, therefore, and no other mode of obtaining food, is the condition of the sum of such characteristics, and of the concourse of so great forces in one and the same animal."

This, gentlemen, is the true Cuvierian style, in which, as in numberless parts of his works, Professor Owen has continued to breathe out the very spirit of the founder of palæontological science.

It is by such labours that geology is steadily gaining a higher place among the sciences. Comparative anatomy has truly been our steadiest auxiliary, and well may we do honour to those who impart to us such truthful records; for, whilst the histories of the earlier beings of our own race are shrouded in obscurity, whilst the first chronicles of ancient Rome and Greece are now admitted to be exaggerated, and often even fabulous, we turn back the leaves of far more antique lore; and, not trusting to perishing inscriptions, mutilated by successive conquerors, and assuming a hundred meanings under the eyes of doubting antiquaries, we appeal only to the proofs in Nature's book, and find that their reading is pregnant with evidences which must be true, because they are founded on unerring general laws.

5. *The chief aim of the Geological Society of London.*—The chief aim of this Society has been to gather sound data for classification; and, following out this principle, I have endeavoured to shew, how the order of succession established in our own isles, is now extended eastwards to the confines of Asia, and westwards to the back-woods of America. From such researches, and by contributions from our widely-spread colonies, we have at length reached nearly all the great terms of general comparison.

Besides ascertaining where the great masses of combustible matter lie, we can now affirm, that, during the earliest period of life, conditions prevailed, indicating a prevalence over enormous spaces—if not almost universally—of the same climate, involving a very wide diffusion of similar inhabitants of the ocean. We have learned, that, in the earliest of these stages of animal life, no vestige of the vertebrata has yet been found; whilst in the succeeding epochs of Palæozoic age singular fishes appear, which, in proportion to their antiquity, are more removed from all modern analogies. In each of these early and long-continued periods, the shells preserving on the whole a community of character, differ from each other in each division—and in that later formation, where a very few only of the same types are visible, they are linked on to a new class of beings, the first created of those Saurians, whose existence is prolonged throughout the whole Secondary period; whilst we have this year seen reason to admit, that even birds (some of them of gigantic size) may have been the cotemporaries of the first great lizards. With the close of the Palæozoic æra we have also observed a gradual change in the plants of the older lands, and that the rank and tropical vegetation of the Carboniferous epoch is succeeded by a peculiar flora. In the next, or Triassic period, we have another flora, whilst new forms of fishes and mollusks indicate an approach to that period when the seas were tenanted by Belemnites and Ammonites, marking so broadly these secondary deposits with which British geologists have long been familiar, and which, commencing with the Lias, terminate with the Chalk. And lastly, from the dawn of existing races, we ascend through successive deposits gradually becoming more analogous to those of the present day, until at length we reach the bottoms of oceans so recently desiccated, that their shelly remains are undistinguishable from those now associated with Man, the last created in this long chain of animal life in which scarcely a link is wanting!—all bespeaking a perfection and grandeur of design, in contemplating which we are lost in admiration of creative power.

Such results, grand as they are—nothing less in short than the records of creation—are, however, but a portion of the labours of geologists. They have also struggled to explain the causes of those great revolutions. In some continents, it is true, the pages in the book of Nature are, as it were, unruffled; for, by whatever agency effected, it is certain that beds of vast ancient oceans have been so equably elevated and depressed, and again so steadily elevated from beneath the sea, that the continuity of their rocky desposits over areas larger than our kindoms of Western Europe is unbroken, and their original condition almost entirely preserved. In other regions, on the contrary, the sediments in the sea and the masses of the land have been pierced by numerous out-bursts of igneous and gaseous matters, accompanied by violent oscillations and breaks, whereby the chronicles of succession have been sorely defaced, and often rendered more illegible than the most carbonized of the papyri found under the lava of Vesuvius. Nay, so intensely has this metamorphism operated, that obliterating all vestiges of former life, and concealing them from us, we have been sorely puzzled to ascertain by what powerful physical agency such mighty changes can have been accomplished,—changes by which the strata have been convoluted into forms grotesque as the serpent's coil, inverted in their order, or shivered into party-coloured and crystalline fragments. And yet in these broken and mineralized masses, as another branch of our science teaches, are found the precious ores, and the metals most useful to mankind.

Such complicated relations and such changes in original structure call forth the application of the highest powers of physical science; and not only involving the agency of that great central heat, to which geologists have willingly referred, but also invoking the aid of agents, some of them still mysterious, by which electricity and magnetism are bound together in the cycle of terrestrial phenomena. To few of us is it given to venture with firm steps into that region; and, though I hope to live to see some of these questions answered, I am well satisfied to have been among you when such solid advances have been made, in deciphering the mutations of the surface of the earth, and in the compilation of a true history of its earlier inhabitants.

Notices of Earthquake-Shocks felt in Great Britain, and especially in Scotland, with inferences suggested by these notices as to the causes of the Shocks. By DAVID MILNE, Esq., F.R.S.E., M.W.S., F.G.S., &c. Communicated by the Author.

(Continued from Vol. XXXIV. page 106.)

Having described in detail, the impressions produced in different parts of Scotland, by the shock of an earthquake felt on 23d October 1839, and some of the most striking circumstances which accompanied it, we shall now shortly notice the results which seem deducible from these details; and then proceed in noticing the shocks which subsequently occurred.

1. The shock in question was *felt* throughout *two-thirds of Scotland.* The southern limit of the region affected, was nearly coincident with a line drawn from the Solway to the mouth of the Tweed. Its northern limit was as nearly coincident with what is known as the Great Glen of Scotland, viz. along the course of the Caledonian Canal. The shock was felt on the east and west coasts, at places comprehended between the southern and northern limits above described.

2. Throughout the whole of the region so affected, the shock appears to have been felt *simultaneously*, or at least so nearly so, that it is impossible to discover any appreciable interval between the observations of the shock in different places.

Little dependence, however, can be placed upon this inference, in consequence of the impossibility of trusting to the ordinary clocks and watches, by which the observations were made.

3. The shock was *not* felt everywhere, with the *same* degree of *intensity*. It was greatly more severe in Comrie, and the immediate neighbourhood of that village, than in any other part of Scotland,—the extent to which walls were rent or thrown down, and the loudness of the attendant noise, being incomparably greater there than any where else.

The intensity of the shocks, at other places, was not in proportion to their distance from Comrie. Thus, in Aberdeenshire, at places exceeding 100 miles from Comrie, the shock exhibited much more violence than in the Lothians, though only half as distant. The lines of equal intensity appear to have formed ellipses, of which Comrie is the centre, and of which the longer diameter is about NE. and SW., or parallel with the chain of the Grampians. This circumstance may probably be accounted for, by the geological structure of this part of the island. If the shocks were caused by vibrations transmitted through the earth's crust, they would be transmitted best where the strata were solid and continuous. The primitive and igneous rocks, forming continuous ranges of hills, running NE. and SW., would facilitate the transmission of the shocks in these directions,—whilst the secondary formations deposited on the sides of these mountain-chains, would obstruct the transmission in opposite directions.

4. The shock was produced by a *movement of the ground*, which, in some places, especially near Comrie, consisted of several distinct and successive undulations, followed by a general quavering, "shuddering," or *tremblement de terre*, but in more distant places, of a *tremblement* only.

Eight of the accounts before given bear, that the shock consisted of *two* undulations, each accompanied by a loud report or subterranean explosion. Six other accounts bear, that there were *three* undulations. There is one which bears, that as many as *four* distinct rockings were perceived.

The height to which the earth's surface appeared to be raised by the shock, was attempted to be estimated by some individuals,—though only of course as matter of impression or conjecture. Thus, it was thought, by persons at Comrie who felt the shock, that the ground was elevated 6 or 8 inches; and in the Carse of Falkirk, 4 or 5 inches. At Callendar, Dr Fogo thought the house was raised "with a violent jerk, 6 or 8 inches from the ground." Dr Stein of Menstrie, near Alloa, says, that his servant thought the kitchen was lifted "up from 2 to 3 inches."

The angle made by the wave with the horizon, appeared to be at Alloa 1° 18′, and in the Carse of Falkirk 3° 47′. If, as an average, the wave is supposed to have been 5 inches high at its apex, and to have sloped on its anterior and posterior surfaces at an angle of 2½ degrees to the horizon, the breadth or base of the wave would have been rather less than 20 feet. But the observations from which these results are deduced, were so few and so casual, that no great value can be attached to them.

That there were undulations formed by the shock, is indubitable. On this point at least, full reliance may be placed on the concurrent sensations of numbers of individuals, several of them experienced naval officers, who attest the fact of there having been a *heave* which lifted every thing upwards and forwards as in a ship at sea. Indeed, on no other supposition is it possible to account for the different effects produced on walls, —those rent vertically, being generally at right angles to those which were rent horizontally, and which last leaned or tumbled over.

One intelligent observer at Crieff (Duncan Brewster), gave to the author the following explanation of his sensations. He felt himself first moved eastward from 1 to 2,—then back to 3,—and, lastly, from 3 to 1.

5. A *sound* accompanied the motion of the earth. At Comrie it is said to have been " indescribably terrific; that of water, " wind, thunder, discharge of cannon, and the blasting of rocks, " appeared combined" in it. At a place half a mile east of Crieff, the shock, of 12th October 1839 was accompanied by " a loud explosion, as if from a ten-pounder at a mile distant." At Lawers (1½ mile east of Crieff), the noise on 23d October 1839, "was exactly like the nearest and loudest peal of thunder; only, instead of being over head, it seemed to rise up immediately under the house." At Dunira (2½ miles west of Comrie), the noise on the 12th October 1839, is described as " most tremendous,—not unlike the blowing up of a magazine." At Monivaird (4 miles east of Comrie), the noise on 23d October 1839 is said to have been " a loud roar, like that of the blast-furnaces in a large iron-work." At Crieff, on the same occasion, the noise is stated by one observer, to have been " tremendous;" and, according to another, " was as loud as if ' a sixty-pounder had been discharged at the bottom of a coal-" pit, 300 or 400 yards away." This same observer adds a most remarkable fact,—" Immediately after the shock, I got " out of bed, and, on looking from my window, beneath which " were a number of trees, I observed that their branches were " all bent towards the *east*, as if a strong gale were blowing " on them. I looked till they recovered their erect position, " after which not a leaf moved. During the time, I heard a

" hollow *sugh* in the air, resembling the draught of a furnace,
" —this continued about 20″ after the concussion."

In most of the accounts from the immediate neighbourhood of Comrie, it is not expressly stated, whether the noise or explosion was in the earth or in the air. But in the accounts from places more distant, it will be seen, that notice is taken of a sound in the air as well as in the earth. The Rev. Mr Coventry compares the former to " a rushing *wind* (though the air was perfectly still at the time), accompanied by a noise as if of cattle or horses running rapidly past the windows." " Besides these, and following them, there was heard a rumbling noise, as if of carts on a pavement, but more hollow in the sound. This *latter* sound was in the *earth*." So also, Mr Syme, the Sheriff-substitute of Kinross, says, that " the shock was preceded by a hoarse rushing noise, something like the wind among trees. It continued for 15″ or 20″. It was not as loud as thunder in general, though not unlike distant thunder." A correspondent at Alloa states,—" The first circumstance that attracted my attention was a sudden and violent gust of wind, accompanied with a more than ordinary rushing noise. The gust of wind and rushing noise preceded the shock."

This aërial sound was perceived to accompany most of the other shocks in October 1839. Thus, on the 9th October, Mr Buchan speaks of the "sound, which, as formerly, somewhat preceded the shaking;" and he describes it as "somewhat resembling one of those winds called *harkening* winds." On the 12th October, the two shocks felt at Comrie on that day " were accompanied with a noise resembling a mixture produced by the rush of the strong wind and the peal of distant thunder." On the same afternoon, a third shock—the most severe of the three—" was accompanied with a noise which at first resembled the murmurings of distant waters. This continued increasing in intensity for about 2″; and then followed a very loud and terrific sound, resembling that of a double shot for blasting rock, immensely charged." A person on the hills north of Blairgowrie, just before feeling this last-mentioned shock, "heard a noise, as if of a large covey of muirfowl flying at a little distance from him."—"Finding that it continued, and seemed approaching him, he stood still, leaning on his fishing-rod, to observe the phenomenon, and heard the sound distinctly approaching, until it seemed to surround him, when he felt the ground tremble under him." "He states the sound as resembling the sound of *muir-burn;* or the rattling, crackling, and hissing noise made by a large extent of heather on fire."

From these accounts, it is plain that there are connected with the earthquake-shock, sounds both in the earth and in the air, and which are apparently quite distinct and independent. It is true that the noise generated in the earth must produce some effect on the air, otherwise it would not be heard; and, therefore, it may be thought that this explains the perception of a noise in the air. But this would be an erroneous inference. If the sound generated in the earth is distinctly perceived to come from the earth, it cannot be confounded with any aërial noise; and it is impossible that the ear should make a mistake, in regard to the place from which the sounds proceed. If this be the case when the sounds are similar, much more must it be the case when they are so dissimilar, as the subterranean and the aërial sounds above described appear to be.

The next question is, What is the *cause* of this two-fold sound?

It is stated in almost all the reports, that the noise precedes as well as follows the movement of the ground; that it gradually dies away; the moment of greatest loudness coinciding with the moment of greatest movement in the ground.

It may be inferred from this, that what produces the movement in the ground produces also the sound in both the earth and air. The former sound may be accounted for merely by the commotion of the strata of gravel and soil caused by the shock. But how can the sound or report heard in the air be produced? What is the particular agent which produces the undulations of the earth's surface, and which, when passing into the atmosphere, produces the peculiar noise heard there? A satisfactory answer to this question would probably go far to explain the cause of earthquake-shocks, at least in non-volcanic countries.

But it is better to abstain from speculation on this point, until the results of other observations, no less material, have been considered.

6. It is very plain that the *earthquake-shocks* described in the foregoing reports, were manifested in their *greatest* intensity *at Comrie*, and its immediate neighbourhood.

There was there greater injury to walls, more displacement of furniture, more loudness and longer continuance of sound, more frequency of shocks, than in any other part of Scotland.

In one sense, the shocks may be said to have *originated* at Comrie, seeing that, at all other places, without exception, where motion was felt or sound heard, both the motion and the sound were perceived to come from that neighbourhood.

But it is an incorrect form of expression to say, that the shocks originated at any point on the earth's surface. It is plain that they emanated from a point in the interior of the earth, below Comrie, or its immediate neighbourhood. This inference is deducible from those observations which shew, that at Comrie the shock was felt to come perpendicularly up; and that, at other places, it was felt to come up in a slanting direction.

Thus, at Comrie, one person says of the shock on 23d October 1839, "I felt the shock strike the ground perpendicularly under my feet three times, like the stroke of a ponderous hammer; and, as far as I can guess, lifted the ground 6 or 8 inches." Another observer, who was half a mile west of Comrie on the 14th October, says, "I felt myself lifted, or rapidly heaved up and down."

At Blairmore, 10 miles NE. of Comrie, an observer, who felt the shock of 23d October, says that "it produced a sudden and fearful heave upwards, with great unsteadiness or quavering motion, and immediately an equally sudden motion downwards, when all appeared to rest as before. The force by which the upward motion of the earth was occasioned did not appear to me to act exactly perpendicularly, but rather in a *slanting* direction;" and he accordingly afterwards describes it as "the *angular* jolt or roll upwards."

These differences in the direction in which, at different places, the shock came upwards to the surface, are all quite intelligible, on the hypothesis offered in a previous part of this memoir,* that it is the effect of a vibration transmitted through the crust of the earth, and they serve, therefore, to confirm the correctness of that hypothesis.

On the same hypothesis, an easy explanation is afforded of a number of other facts, to be found in the reports before quoted. Thus, the clattering of slates on house-roofs, would be produced by the transmission of vibrations upwards through the earth's strata and the solid walls of the building. The projection of masses of rock from the south slopes of the Ochils, Damiat, and Castle Rock of Edinburgh (all of these places being to the south of Comrie), is, on the same principle, quite intelligible. For the same reason, at places situated east of Comrie, the *east* gables of houses were necessarily the most damaged,—whether by being rent horizontally, or by being made to lean over towards the east:—the west gables, by abutting against the front and back walls, were much better able to withstand an impulse from the westward. Hence, also, the general violence of the shocks was more felt by people in the upper, than in the lower flats of houses.

In a great number of the reports, it is remarked that the undulations were most striking in carse or alluvial grounds, though the accompanying *noise* was not so great there as in rocky ground. If the noise alluded to, be that produced in the earth, it is obvious that there would be none where there were no materials capable by attrition of producing noise. With regard to the undulations, they would obviously be more easily formed in places where the materials were flexible, and thus capable of yielding to impulses from below.

7. Among the accounts of the shocks which occurred in October 1839, there will be found observations of some ano-

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* See Ed. Phil. Journ. vol. XXXI. p. 276.

malous phenomena, chiefly of a meteorological character, and apparently connected with these shocks, which it is proper not to lose sight of.

(1.) There is the circumstance mentioned by Sir John Mansel, that, on the morning following the great shock of 23d October 1839, some linen clothes were found entirely covered with a *black powder*. This circumstance is interesting, on account of a similar powder which, on several occasions, both before and afterwards, was observed on Loch Erne, and the adjoining country.

The previous occasion* on which this black powder was observed, has been already mentioned. It may be as well to notice in this place other occasions, when it was again remarked, and to mention the result of an examination of it which Professor Traill kindly undertook.

A few days before the 23d October 1839, some boatmen on Loch Erne observed on the surface of the water, a scum as black as ink, which rendered it unfit for domestic use.

In November 1839, there fell over the district at and near Comrie, a powder blacker than soot, and which defiled linen clothes on the hedges. Dr Forbes, a medical practitioner at Comrie, informed the author, that he had seen this powder at various places, particularly on the public road opposite to Dunira, where it was in considerable quantity. A good deal of rain had fallen shortly before Dr Forbes observed the powder, and in some parts of the road he observed the powder floating on the pools of water.

On the 23d February and 2d March 1841, a quantity of black powder was observed on Loch Voil and Loch Erne, as shewn by the following letters from Mr Stewart of Ardvoirlich. It may be mentioned that Ardvoirlich is situated on the south bank of Loch Erne, near the west end, and about twelve miles west of Comrie :—

"ARDVOIRLICH, 1st *March* 1841.

"DEAR SIR,—On Tuesday morning last Mr Macdonald of Cragnie observed a *black scum*, which he mentioned to you here, he had frequently seen on Loch Voil. He collected two bottles of it, which he requested I would forward to you. Mr Macdonald observed it first at the west end, and then in several places along the shore. He described it as having an oily appearance, and shining on the surface of the water, and, when thrown out by the waves, formed bubbles like soap-bubbles. When he put his hand in it, it made it quite black. It has every appearance of being the same substance as appeared on Loch Erne in February 1837. There had been no rains for five or six days previously, and there being but little snow on the hills, the hill-streams were not in the least swollen, so that it could not have come into the Loch in that manner. On the morning on which Mr Macdonald observed it, there was a slight frost, and the morning was very calm, and a thick fog hung over the loch till towards noon; after that, a breeze of wind sprung up, and the scum was soon dispelled.

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* Vol. xxxi. p. 121 and 292.

"We have felt no earthquakes in this quarter this season.—I remain, Dear Sir, yours very truly,

"Robert Stewart."

"Ardvoirlich, 20*th August* 1841.

"My Dear Sir,—As to the scum found on the Lochs in this neighbourhood, I must say that I have not been able to *ascertain* that the black powder was ever seen but on the surface of the Lochs, although I certainly heard it alleged in 1837, when it was first observed on Loch Erne, that some clothes which had been left out all night, had been soiled by something which had fallen on them; but I do not think that this is well authenticated.

"In reply to the second query, I may mention, that I think the very day after I sent off the Loch Voil water to you, I observed a small quantity of the scum on Loch Erne, near the east end. I was then on my way to St Fillans, and intended on my return, to procure a vessel for securing some of it; but before I returned, a strong breeze of wind had dissipated it, and I have heard of none being seen since. I think I mentioned to you, when I sent you the scum from Loch Voil, that it was observed on a very calm morning. The day on which I observed it on Loch Earn, was also very calm in the morning, attended with a thick haze on the Loch.

"Should I hear or see any more of the powder in any shape I shall endeavour to procure it, and send it to you.

"I happened, unfortunately, to be at Perth on the 30th July, and was not myself sensible of the shock on that day. It was distinctly felt here; but it was not so severe as the shock in October 1839, although all concur in saying that it continued longer. Some describe the sound as appearing to have travelled from south-west to north-east. I would be inclined to infer also, from the accounts I have received from different persons, that this one was more severely felt on the *higher* situations than on the *lower*."

One of the bottles mentioned in Mr Stewart's first letter, containing the powder from Loch Voil, was given to Professor Traill for chemical analysis. His report is in the following letter:—

"10 Albyn Place, 12*th August* 1841.

"My Dear Sir,—On carefully evaporating, by a gentle heat, eight fluid ounces of the water of the loch, it left behind a black residuum, which, when thoroughly dried, weighed 4.48 grains—so that each English pint may be considered as containing nine grains of black matter. This, when chemically examined, appeared to consist chiefly of carbon, with minute portions of argil and silica, and, I think, a trace of iron; but having mislaid the note of the relative quantities, I am unable to give them accurately—and I intend, early next winter, to repeat the analysis, if I do not recover my notes.

"The black matter is in a state of extremely minute division, for it took several months entirely to subside, leaving the water of a very pale wine colour, and transparent. The black matter attached itself to the sides of the containing glass-vessel, so as to give the contained fluid an inky hue. The greatest portion had subsided to the bottom, and the fluid itself was of the colour above mentioned when drawn off.

"The origin of this dark matter is rather puzzling. Were it only seen after heavy rains, it might be supposed owing to water charged with the finer particles of decayed vegetables from the adjacent peat-bogs.

"The light pulverulent appearance of some of the floating particles, would incline one to suspect that it was the product of combustion. The extent of the phenomenon, and its occurring at different seasons, will scarcely admit the practice of burning the heaths as the cause. It is true that the carbonaceous particles might be lodged on the moors for some time, and transported by high winds a considerable period after the burnings.

"Can it be the product of any species of *sublimation*, caused by subterranean heat? Its frequent occurrence in a district so often shaken by earthquakes, which has so evidently sympathized with remote volcanic action, would rather favour this hypothesis.

"The volatilized carbon might find its way, on the wings of gaseous emanations, through fissures too minute to afford the phenomena of smoke; and the source of the internal heat might be too distant to give rise to any other volcanic phenomenon.—I am, my Dear Sir, very truly yours,

"THOS. STEWART TRAILL.'

"DAVID MILNE, Esq."

In regard to the origin of this black powder, it would be perhaps premature to form a very decided opinion till the relative proportions of carbon, argil, silica, and iron, existing in it, have been communicated. That it comes from the atmosphere, however, seems already quite clear. That it is not derived from the smoke of houses or muir-burnings, seems also to be made out. May it not be derived from the surface of the ground, the finer particles of which are, it is well known, very frequently carried up into the atmosphere by whirlwinds, and even by evaporation only?

In the Comptes Rendus for 1841, two occasions* are mentioned, when, first at Vernet (in the eastern Pyrenees), on the 17th February 1841; and second, at Genoa on the 18th February 1841, rain fell, charged with dust; and it is a remarkable coincidence that, on the last of these occasions, there was an earthquake-shock. It is stated in this last account that the dust fell, not only on the day of the shock, but on that preceding and on that following it; and that for about a month previously, there had been an enormous quantity of rain, and a very low barometer. The dust which fell, both at Vernet and at Genoa, was analyzed, and found to consist of the ingredients composing the porphyritic rocks of the country, the quantity of each ingredient bearing a tolerably near proportion to the relative quantities in the rocks themselves.

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* C. R. p. 62 and 218.

The opinion mentioned in the Comptes Rendus, as entertained by those who made the analysis and attended to the phenomenon, was, that by a "trombe d'eau," or some such phenomenon, the debris of the rocks had been carried up into the atmosphere in a state of minute division, and then brought down by rain. Is a similar explanation not applicable to the black powder which falls in the mountainous districts of Perthshire? The clay-slate formation undoubtedly possesses all the elements except one, of which Dr Traill found the powder to be composed; and this one element, viz., carbon, which is not in that rock, may readily have been derived from the peaty covering of the hills. How these particles were carried up into the atmosphere, it is not difficult to conceive; for, besides the suction of an ascending water-spout, as suggested in the Comptes Rendus, there must be often in these mountainous regions, whirlwinds or eddying currents of air quite capable of carrying up earthy matters; and, indeed, the mere effect of evaporation from the soil would be sufficient for that purpose.

If these views be correct, there is no connection, at least of a direct nature, between the falling of the black powder and the occurrence of earthquake-shocks in Perthshire. It is true that water-spouts and whirlwinds are connected with sudden changes in the electrical state of the atmosphere; and thus there may be a remote connection between these phenomena and earthquake-shocks, if the latter are attributable to electricity: but this is a point which has not yet been established.

(2.) The next circumstance deserving notice, is a peculiar *smell or odour,* which was perceived by many persons at or about the time of the earthquake shocks in October 1839.

Thus, Sir John Mansel, who felt the great shock of 23d Oct. 1839, in Comrie House, says, that, towards the termination of it, "there was a strong smell of a combination of a sulphureous and metallic air emitted through the floor." At Kingussie, "there was a sulphureous smell at the time of the shock, which was most distinctly felt by the Rev. Mr Shepherd, who went out almost immediately after the shock, and called Mrs Shepherd, who felt it very strong." "It was also perceived about twelve miles farther up the river (Spey), by persons, who describe it as having the smell of the washings of guns." At Woodcot, near Dollar, Mr Walker says, that "The weather on the day of the shock, and also the one preceding it, was uncommonly calm—very foggy towards evening, and the air at that time felt much warmer than the degree of heat indicated by the thermometer; and I thought (but it may have been fancy), that there was a peculiar odour perceptible. In 1824, when at Lisbon, I perfectly recollect having remarked the same thing."

On making enquiry as to this odour, when he visited Comrie shortly after October 1839, the author found that it had been remarked by various individuals, on several different occasions during that month. But he could obtain no very precise information as to the nature of it. It will be observed from the letter of Mr Cameron, the parochial schoolmaster of Comrie, before quoted, that he at first discredited the statements of individuals as to their having perceived a smell. He says, that "There was a sulphureous smell perceived, which was supposed to come *off the river*: but several persons, at a *considerable distance from* the river, were sensible of it. Though I heard of many who said they perceived this, I would not believe it, till one evening Mrs C. was standing at the water-edge, and wishing to convince me of its reality, asked me to go to the water-edge. I did so, and I must say that the smell was so strong that it could not be mistaken. It was a little after sun-set, and the evening having suddenly turned to intense frost, the whole course of the river could be traced from a distance, by a sort of vapour or mist that arose from it—most favourable for giving out any effluvia."

Mr Colquhoun of Clatheck, also assured the author that he had, on the 23d October, perceived the sulphureous smell; and he heard that it had been taken notice of in Glenlednock, as well as in other parts of Stratherne. It is, in these circumstances, very difficult to doubt the existence of a smell or odour, occurring simultaneously with these earthquake-shocks, attested as it is by so many respectable individuals—many of them apart from, and independently of, each other. The only point on which reasonable doubt may exist is, whether it had any connection with the shocks. Might it not have been produced by some other, some accidental cause? But it would be a very curious accident indeed, which caused the same sort of smell to be perceived in Stratherne, on the Spey, and on the south side of the Ochils, and many other places, all about the same time, and in such circumstances as to produce a conviction in the observers, of its being somehow connected with the earthquake-shocks. Nor should it be forgotten that a smell or odour described as sulphureous, has been very frequently perceived after lightning. Nay more, means have recently been discovered, of producing precisely the same odour by electrical action. Professor Schönbein of Bâsle, in a Memoir published in the Transactions of the British Association for 1840,* details a number of experiments on this subject, shewing that water, when decomposed by electricity, gives out the odour above referred to; and he expressly states, that when it is perceived after a flash of lightning, it is owing to the decomposition of the vapour or moisture in the atmosphere.

If, then, it should appear, in the course of farther investigations, that, during the occurrence of earthquake-shocks, a large supply of electricity really passes from the earth into the atmosphere, we shall cease to wonder that a sulphureous smell was perceived, when the shocks in Scotland were most frequent and severe.

(3). In many of the reports which have been quoted, notice is taken of impressions still more peculiar, as connected with the earthquake-shocks.

A feeling of *nausea* was experienced by many individuals, and which is variously described as resembling "sea-sickness,"—"sickness, like that felt before fainting"—"uneasy sensation, which I can compare only to the first disagreeable feelings which usually precede a fit of sea-sickness,"—"a most peculiar sickish sensation, such as I never felt before."

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* Page 218.

Headaches were produced, as attested by the Rev. Mr Walker on the 12th October, by Mr Rutherfurd, W.S., on the 14th October 1839, and by Mr Young of Crieff, on the 23d October 1839, all of whom ascribe these as the effects of shocks which occurred on these days.

Nervous sensations of a more indefinite kind are spoken to by various individuals. On the 14th October 1839, at the moment that the shock occurred, an individual, though he was not aware of its occurrence, experienced "an unusual feeling, which led him to suppose that some illness was impending." Mr Robertson, who felt the shock of 16th October at Glendevon, on the north side of the Ochils, says,—"I remember having just before, felt as if some strange presence had been silently gathering round me, and could not be shaken off." Mr Laurie, the parish schoolmaster of Monzie, says, "the shock of the earthquake on 23d October, affected the nerves disagreeably, and left a painful impression. It reminds me vividly of the shock from an electric machine."

The conviction of there having been an electrical discharge, was decidedly entertained by a number of individuals. Thus, at Alva, near Tillicoultry, two clergymen felt as if electrified. Mr Jeffrey, who felt the shock in the Carse of Falkirk, says,—"I may mention a circumstance which I have not seen taken notice of in any account of the late earthquake, and it is, that I am convinced it was accompanied with an electric shock. I was perfectly calm and collected at the time when it came on, and never had any doubt of what it was, nor was I at all alarmed for the consequences. But the feeling produced upon my body, was exactly similar to what an electric shock has in other circumstances had upon me." Mr Stein, surgeon at Menstrie, near Stirling (in a report not before quoted), says, "I think the atmosphere (on the 23d October 1839) was highly charged with electricity, both before and at the time when the shock occurred." He speaks of "the slightly reddened or lurid appearance of the atmosphere towards the S. and SE., particularly observable for several evenings preceding the shock of the 23d. In the afternoon of the 23d, I observed this appearance so distinctly as to mention it. Later on in the evening, this appearance could not be observed, in consequence of the misty and wet night which set in hereabouts." He speaks, also, of his "own feelings, and those of several of my patients, in reference to headaches, nervous excitement," &c., which he attributes to an electrical state of the atmosphere.

In regard to the electrical state of the atmosphere in October 1839, it is of course impossible to draw any very certain inference. It is, however, remarked, in several of the reports before quoted, that the aurora borealis and shooting stars were more frequent than usual in September and October 1839. Farther, on the 23d October, as Mr Craigie of Dumbarnie informed the author, "one of my family is quite positive she saw a flash of light during the shock; but this was not observed by any of the others present."

(4.) Every one who has read the reports quoted in the previous part of this Memoir, must have been struck by the frequent allusions made to the rainy state of the weather during the occurrence of the principal shocks in October 1839, and for some weeks previously. The Rev. Mr Walker of Comrie says, that the flood in the river Erne on the 15th September 1839, was greater than any which had occurred for 30 years;* and the author has now before him reports by other inhabitants of the district, to the same effect.

Knowing, however, that in matters of this kind there is much risk of mistake, the author has been at pains to obtain accurate information as to the quantity of rain which actually fell in different parts of Great Britain in 1839, and particularly in August, September, and October of that year, and also as to the quantity usual in the same places, shewn by the fall in previous and subsequent years.

For this purpose, he obtained extracts from above twenty meteorological registers kept in the south of Scotland and north of England, and with these he has compiled a table, shewing the average quantity of rain which fell in each year between 1835 and 1842, inclusive. The following is the quantity† for each year, in inches:—

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* See vol. xxxii., p. 123.

† The table shewing these results is omitted from want of room.—Edit.

Years,	1835,	1836,	1837,	1838,	1839,	1840,	1841,	1842.
Inches,	33.6,	42.6,	33.9,	25.1,	42.8,	37.1,	34.1,	22.1.

Thus, it will be seen that, in the year 1839, a greater quantity of rain fell than in any of the four previous years, or in any of the three which have followed it. The average quantity for the whole of these nine years was 30.1 inches,—whilst the quantity which fell in 1839 was 42.8,—being more than one-third of an increase.

Farther, on comparing the quantity of rain which fell in the districts from which the above returns were obtained, during the months of August, September, and October, it appears that, whilst in 1839 there fell, during these three months, 12.46 inches of rain, the quantity which fell in the same three months of the preceding year was 9.86 inches, and of the succeeding year only 6.73 inches.

Mr Luke Howard, in his recent publication on the meteorological character of the last eighteen years, in Great Britain, observes, that "the year 1839 was the wettest of the whole cycle, with the lowest barometer." The Rev. Mr Dunbar of Applegarth, in Dumfriesshire, who keeps a most accurate Meteorological Register, observes, in regard to the year 1839, —" The quantity of rain is greater than has fallen any year for at least 13 years—the mean of that period being 35 inches,"—whilst, according to his register for 1839, the quantity of rain was 43.29 inches.

At Kinfauns,* near Perth, the seat of Lord Gray, where two rain-gauges are observed and registered with great accuracy, there fell, in two days only (viz. between the 22d and 24th October), no less than 1.88 inches of rain, being $\frac{1}{17}$ of the quantity for the whole year. The quantity of rain which fell there in 1839, was 32.23 inches. Now, according to a report by Dr Anderson, on the quantity of rain† which falls at Perth (situated only two miles from Kinfauns), " the *maximum* " quantity of rain for a period of 16 years (previous to 1835), " is 31.01 inches." This corroborates Mr Luke Howard's statement as to the very rainy character of the year 1839.

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* Kinfauns is about 22 miles east of Comrie.

† British Ass. Reports for 1840, p. 57.

These extracts from registers verify, therefore, the general, or rather the universal remark, made by all whose observations have been founded on, as to the unusually large amount of water with which, for weeks or months previous to the commencement of the shocks, the earth was saturated.

But here another fact, of very material importance, which forcibly arrested attention in Perthshire, must be specially adverted to. It was remarked, that, flooded as the rivers were, they were not so much flooded on the 23d October 1839 as might have been expected, considering the great abundance of rain which had fallen. The Rev. Mr Walker adverts to this circumstance in the passage of his report above referred to. The same was remarked of the river Airdle,* near Blairgowrie, and of the rivulets at Dunira, two miles west of Comrie.

It is also not a little remarkable, that in Perthshire and other parts of Scotland, wells and springs of water, were in the month of October, notwithstanding the quantity of rain which had fallen, in many places much diminished, or altogether dried up. In one of the accounts received, it is stated that "the water diminished more than one-half in the springs about Crieff." At Cawdor, in the county of Nairn, a pump-well went dry on the 24th October. "A field" (on the farm of Park, situated about 2 miles south of Nairn), which at this season was always covered with water, that could not be drained off on account of the granite, is now quite dry, and was ploughed a few days ago." At Inveragle House, near Elgin, the servants had, for a day or two after the shock, to go half a mile for water, in consequence of its disappearing from the well usually resorted to. At Ballimore on Loch Fine, a well went dry permanently, which always previously had afforded abundant supplies.

These facts are curious. It is true, that October is the month when, in Scotland, springs often diminish in quantity; but this arises from the dryness of the weather which generally occurs in that month and shortly before,—an explanation quite inapplicable to the year 1839.

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* See Vol. xxxiii., p. 388.

Two facts, then, seem to be established : *first*, that a very unusual quantity of rain fell in the year 1839, and especially during the months of August, September, and October ; and *second*, that much of this rain must either have sunk down into the interior of the earth, or have reascended into the atmosphere by rapid evaporation.

(5.) Several of the observers, whose accounts have been quoted, and especially the Rev. Mr Walker of Comrie, take especial notice of the fogs or mists which prevailed to an unusual degree in Stratherne, when the earthquake-shocks prevailed in October 1839. And this circumstance, unimportant as it may at first sight appear, is interesting, on account of its being a common attendant on earthquakes.

In proof of this remark, it is only necessary to refer to the remarkable fog which, for some months in 1783, overspread the whole of Europe, and part of Asia and America. "It was "perfectly dry, and did not deposit humidity. During its "continuance, the electricity of the air was increased, and "there happened a great deal of thunder and lightning. At "that time, Mount Hecla, in Iceland, was in a state of erup-"tion ; and it is imagined that it had its origin from that "source."*

It will be perceived from the notices formerly given of the Lisbon earthquakes, and of the series of shocks felt at Chichester in 1834, that this phenomenon occurred in such a way as to create particular attention.

(6.) It is proper to take some notice of the *height* of the *barometer* at and preceding the time when the shocks occurred, in order to ascertain whether the atmospherical pressure was greater or less than usual.

It is unfortunate that, in October 1839, there was not in Comrie or in its neighbourhood, any barometer, regularly observed and registered ; a want which has now been supplied by the liberality of the British Association, at whose expense a barometer, thermometer, and rain-gauge, have since been established at Comrie, and the indications of which are regularly recorded. It so happened, however, that whenever the great shock of 23d October 1839 occurred, Mr Williamson of Lawers, bethought himself of instantly examining the state of the barometer. He found that it had fallen a whole inch since the forenoon of the same day when he had set it,—being an interval of ten hours. Struck with this circumstance, Mr Williamson continued for about two hours after the shock, that is, till after midnight, to watch the barometer. During that time it still continued falling, but after *that*, it began to rise, and continued to rise all next day.

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* Robertson on the Atmosphere, vol. i. p. 229.

The observation thus made by Mr Williamson is of importance, on account of its being made within two miles of the very spot where the shocks are most intense; and it is fully confirmed by the returns obtained from more distant places. The following hourly barometric heights shew, that, at Kingussie, (about 50 miles north of Comrie, and 700 feet above the sea), the atmospheric pressure was least about 1 A.M. on the 24th October:—

1839.	October	23.	at 8	P.M.	29.136
...	......	...	... 9	...	29.122
...	......	...	... 10	...	29.100
...	......	...	... 11	...	29.092
...	......	...	... 12	...	29.084
...	......	24.	... 1	A.M.	29.080
...	......	...	... 2	...	29.120
...	......	...	... 3	...	29.142
...	......	...	... 4	...	29.156
...	......	...	... 5	...	29.200

At Inverness, the barometer, which is there also hourly registered for the British Association, began to fall at 4 A.M. on the 23d October, and reached its minimum about 5 P.M., but had risen again only .044 when the shock was felt.

The barometric registers kept at Dollar Academy (30 miles SE. of Comrie), and at the Cameron (50 miles SW. of Comrie) which state the atmospheric pressure at a fixed hour every morning and evening, also agree in shewing that the barometer was lower on the night of the 23d October than it was in the forenoon of that day, or for some days before and after it.

Thus, then, it appears that, when the shock of 23d October 1839, or rather the series of shocks of that night occurred, the pressure of the atmosphere on the part of the earth's surface affected by them, was undergoing very considerable diminution; that the barometer had been falling for several hours previously, and that it reached its lowest point very shortly after the principal shock of that night was felt.

It would have been important to have had the means of obtaining similar information at the time that the other shocks occurred, on previous occasions in the same month, but from the want of any barometers in the district then regularly observed, this is impossible.

It is, however, worthy of remark, that the atmospherical pressure for the year 1839 was considerably lower than usual, and especially in September, as the following results pretty clearly indicate :—

1*st*, In regard to the mean *annual* pressure, it was rather, though not much, less in 1839, than for several years previous and subsequent.

2*d*, In regard to the oscillations of the barometer, as shewn by the mean and maximum ranges in 24 hours, they were greater in the year 1839, than in either the previous or subsequent year.

3*d*, In regard to the absolute height of the barometer, it sunk lower in the year 1839, than it had done for many years before, or has done since.

4*th*, In regard to the particular month in which, during the year 1839, the barometer sunk lowest, all of the registers except one agree in pointing out September.

These observations, as to the amount of atmospherical pressure, are important for two reasons. In the first place, they are in accordance with similar observations made in volcanic countries; and especially in South America. In the second place, they are in accordance with the hypothesis, that during the occurrence of earthquake-shocks, there is an unusual quantity of electricity passing from the earth into the atmosphere. For it has been very satisfactorily shewn by Mr Snow Harris, that the tendency of an electrified body to give off electricity increases, as the pressure of the air upon it diminishes. In order to ascertain this point exactly, he placed in a glass receiver, capable of having the air exhausted from it, two metal balls,—one of them charged with electricity, in order to ascertain the distance or interval at which the spark would pass from the one ball to the other. From these experiments, he deduced the two following laws.

(1.) "The respective quantities of electricity requisite to pass a given interval, varied in a simple ratio of the density of the air. When the density was one-half as great, the discharge occurred with one-half the quantity accumulated;—that is to say, with one-fourth of the intensity or free action."

(2.) "The distance through which a given accumulation could discharge, was found to be in an inverse simple ratio of the density of the air, the intensity or free action being supposed constant. In air of one-half the density, the discharge occurred at twice the distance."*

According to the general laws thus deduced, electricity from the earth will have a greater tendency to flow into the atmosphere, when the atmospheric pressure on the earth's surface is diminished; and if, at the same time, the atmosphere, as well as the ground, be saturated with moisture, then the flow of electricity will be still more facilitated.

The general law established by Mr Harris, and the application of it, now proposed, to the electrical condition of the earth, completely accords with the views before alluded to, of some recent writers who maintain, that, when the atmosphere is in a moist and highly electrified state, the barometer must necessarily be low. "If," says Mon. de Tessan, "a mass of air placed at the surface of the earth is moist and electrified, the barometer ought, *cæteris paribus*, to shew a less pressure than if it were not in that mass."† This is not the place to explain the grounds of the opinion now referred to; it is sufficient to notice the fact, that it is entertained, and by persons whose character and researches entitle them to respect.

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* Snow Harris on some elementary laws of Electricity, Lond. Roy. Soc. Trans: for 1834, p 228.

† L'Institut, 10th June 1841.

Experiments on the explosive force of Oxygen and Hydrogen Gases. By JAMES JOHNSTON, Esq. Communicated by the Author.

To the Editor of the Edinburgh Philosophical Journal.

SIR,—In 1841 I took out a patent for obtaining motive power from the explosive and condensing properties of oxygen and hydrogen gases. In order to ascertain the power and length of stroke which those gases would give when exploded in a cylinder, I commenced, on the 24th April 1841, a set of experiments, of which I now give the results.

The apparatus with which I made the experiments consisted of a strong cast-iron cylinder accurately bored, so that its diameter was exactly two inches and thirteen-sixteenths of an inch. This diameter gives a surface on the piston of six square inches.

The piston was fitted very accurately into the cylinder. I have ascertained it to work perfectly air-tight. On the top of the piston there is a cross-head and spindle for placing weights upon. The ends of the cross-head work in cast-iron guides.

The gases are admitted to the cylinder by stop-cocks, and are exploded by an electric spark.

I shall now describe the preliminary arrangements made before making each experiment.

I ascertained the weight of the piston, piston-rod, and other appendages, which the gases must move when the piston is put in motion, to be 9 lbs. 5 oz.

I then ascertained, that, to overcome the friction of the piston, it required 5 lbs. 1 oz., together with its own weight; therefore add 5 lbs. 1 oz. to 9 lb. 5 oz., and we have 14 lbs. 6 oz., the weight or force required to overcome the friction of the piston.

I now proceeded to load as follows the piston, so that I would give the gases 5 lbs. per square inch of weight to lift.

Weight of piston,	9 lbs.	5 oz.
Amount of friction of piston,	14 ...	6 ...
Amount of weight required to make up the 5 lbs. per square inch,	6 ...	5 ...
Total weight,	30 lbs.	0 oz.

This gives 5 lbs. per square inch of weight, as there are six square inches of surface on the piston.

I measured the gases in the cylinder by the height to which I raised the piston. Every inch of distance between the bottom of the cylinder and the bottom of the piston holds six cubic inches. When making the experiments I always raised the piston to the height which I wished it to be at, by placing under the ends of the arms of the cross-head pieces of wood made for the purpose. After the piston was thus raised to its required height, the apparatus was ready for the explosion, as the gases were admitted at the pressure of the atmosphere at the time the piston was raised.

The gases were kept ready for use in a bladder mixed in the proportions of two parts of hydrogen to one of oxygen.

Having described the arrangements for insuring accurate experiments, I now give the results in the following table, of which the first column gives the quantity of gas in cubic inches which was placed in the cylinder at each experiment.

The second gives the weight that was placed on the piston in pounds per square inch of its surface.

The third gives the height in inches and tenths of inches to which the explosion drove the piston.

The fourth gives the height of the barometer at the moment each experiment was made.

The fifth gives the height of the thermometer at the same time.

Gas	Weight.	Height		Bar	Ther.	Gas	Weight	Height.		Bar.	Ther.
		In.	Ten.					In	Ten.		
6	5	1	8	29 4½	53½	21	20	2	7	29.5¾	54½
9	5	2	9½	29 4½	53½	6	25	0	3	29 5¾	54½
12	5	4	4½	29 4½	54	9	25	0	4½	29.5¾	54
6	10	1	0	29 4¾	54	12	25	0	8	29 5¾	54
9	10	1	8	29 4¾	54	15	25	1	1½	29 5¾	54
12	10	2	5½	29.4¾	54	18	25	1	4½	29 5¾	54
15	10	3	0½	29 4¾	54	21	25	1	7½	29.5¾	54
6	15	0	7	29 5	54½	24	25	2	2	29.5¾	54
9	15	0	9¾	29 5	55	6	30	0	1½	29.7¼	57
12	15	1	6	29.5	55	24	30	1	1	29.7½	57
15	15	2	1½	29 5	55	24	45	0	3½	29.8½	52
18	15	2	8½	29.5	55	24	50	0	1¾	29.1	43
6	20	0	3¼	29 5¼	54½	24	55	0	1	29.1	44
9	20	0	7¼	29 5¼	54½	24	60	0	0¼	29.0	45
12	20	1	1¼	29.5¼	54½	24	65	0	0⅛	29.0	45
15	20	1	5¾	29.5¼	54½	24	70	0	0⅒	29.0	45
18	20	2	2	29 5¾	54½	24	75		0	29.0	45

In the last experiment, viz., that in which twenty-four cubic inches of gas were exploded under a load of seventy-five pounds per square inch, the explosion was unable to lift the piston; it merely shook the weights.

The above table gives the maximum results of upwards of two hundred trials or experiments, which I have made on the explosive force of the mixed gases.

In order to shew that there is an unaccountable irregularity in the results of my experiments on the gases, I shall now give a few experiments which were made with the same gases, and under the same circumstances.

Gas.	Weight	Height.		Bar.	Ther.	Gas.	Weight.	Height.		Bar.	Ther.
		In	Ten.					In.	Ten.		
6	5	1	4½	29.4½	53½	6	5	1	5½	29.4½	53½
6	5	1	5	29.4½	53½	6	5	1	7½	29.4½	53½
6	5	1	3½	29.4½	53½	6	5	1	8	29.4½	53½
6	5	1	3	29.4½	53½	6	5	1	5	29.4½	53½

In the above eight experiments between the maximum and minimum rise of the piston, there is a difference of five-tenths of an inch. How this difference arises I am at a loss to know. A difference of about the same extent existed throughout all my repetitions of experiments. I have bestowed a great deal of labour and attention to find out how this difference arises, and I am satisfied that it has not its origin from any defect in my apparatus or arrangements. I believe it arises from the difference of strength that may exist between the different sparks of electricity with which the gases were exploded, as it was with the spark from a Leyden jar with which I exploded the gases. I intend making a set of experiments, in order to ascertain this point.

When commencing those experiments, I attempted to explode the gases by the spark which is formed when contact is broken between the wires of a battery; but I found that this spark, although very bright, would not explode the gases. The battery which I used for this purpose was composed of eight narrow cast-iron troughs, with a plate of zinc in each measuring twelve inches square. I remain yours truly,

JAS. JOHNSTON.

WILLOW PARK, GREENOCK, 15*th March* 1843.

Some considerations regarding the comparative normal position of Bivalve Molluscous Animals. By M. ALCIDE D'ORBIGNY.

After all that has been written on the position of a bivalve, we would naturally expect that men of science should be agreed as to this important point; such, however, is by no means the case, and the examination I propose to make of the different methods employed will prove this but too clearly.

Linnæus, Bruguière, Lamarck, and Bosc, termed the side on which is the ligament, the base, and according to them the gaping portion of the valve is above; it is the upper side.

M. de Blainville considers a bivalve in a position diametrically the opposite of that adopted by the authors just quoted; thus, the upper side of Lamarck becomes the lower side of De Blainville.

M. Deshayes follows neither the one nor the other of these methods; he reverses the shell altogether, so as to place the side where the tubes are below, and the side where the mouth is above. According to him, the side of the mouth is anterior, the side of the tubes is posterior; the length, however, is the same as that adopted by M. de Blainville.

An examination of these various systematic positions, with reference to the normal position of bivalves, has proved to me that they are all more or less faulty. Every one who studies the shells in their natural position, finds that a *Solen*, a *Mya*, a *Pholas*, and even a *Venus*, invariably have their tubes projecting upwards to the surface of the sand, from the mud or the rock which contains them. Hence it results that the artificial position assigned by Lamarck, differs completely from the natural condition of bivalves, for it forms an angle of 90° with it; and that the position adopted by M. Deshayes differs from the natural one by no less than 180°, or, in short, precisely reverses the shell, by putting below what in the normal state is above, just as if we were to place a man with his feet in the air. As to the position adopted by M. de Blainville, it approaches more to the ordinary condition of the shell, for, by inclining it a quarter of a circle, we restore matters to their natural condition.

Of all these artificial positions, the farthest removed from the truth is that adopted by M. Deshayes. Its author supports it by saying, that the mouth is situated at the extremity, which he places above, while the anus is thus behind. If we were to follow a purely systematic course with regard to the position of animals, without paying any attention to their normal state, we should arrive at the most absurd consequences. For example, is it necessary for us, because man in his usual position has a vertebral column which follows a vertical line, and because he carries his head at the upper extremity of that line, is it necessary, I say, on this account to place the other mammifera in an analogous position? Certainly not, and no one, I believe, has yet thought of changing their normal position, any more than of reversing an echinus by placing the mouth upwards, and the anus downwards,—a position contrary to nature. It is, in my opinion, necessary under all circumstances, to assign to animals in the figures representing them, a position analogous to that which they usually possessed in the different phases of their existence.

Such considerations have induced me to investigate the reasons which have given rise to the singular positions assigned to the mollusca, and the injurious consequences which may thus result to science.

As I have remarked regarding the Gasteropodous mollusca, the special study of shells, *conchology*, having for a long period been considered as a separate branch of the science which treats of the molluscous animals forming the most essential portions of these very shells, there has resulted an erroneous manner of viewing them, to which, however, we have been accustomed up to the present time. We may even assert that this fact is so general, that, including museums, there are more than nine-tenths of our collections which contain none of the animals,—a state of matters which tends to perpetuate the false direction given in the most recent works, where not an animal is figured, and merely the calcareous coverings are represented.

It has never been proposed to change the normal position of birds, or of quadrupeds, because we see them everywhere, and the least observant eye is accustomed to it. The natural position of a bivalve molluscous animal is far from being so well known, for scientific men themselves differ on the subject. Being in possession of numerous shells, and a few animals, authors have fixed a position in their cabinets, either according to the form of the shell itself, as is the case with Linnæus and Lamarck, or, according to zoological characters, as in the case of Deshayes; but without consulting Nature to ascertain if the positions, thus arbitrarily assigned, are in accordance with her.

I have said that injurious consequences might result to science from the representation of shells in a position contrary to nature, and I prove my assertion in the following manner. In order to ascertain if strata have undergone changes of position, if they have been formed at the bottom of a basin, or on an ancient shore, geology and palæontology constantly require information, as to whether organic bodies, and especially acephala, or bivalve mollusca, are in their normal position, and as to whether they have been rolled about or merely displaced. Now, how can a geologist arrive at a conclusion on such points, by consulting, for example, M. Deshayes' treatise on conchology? As the plates in that work represent bivalve shells in a position completely the reverse of their normal one, he would naturally conclude that all the shells he meets with in strata, have undergone a change of position, because there is no one in the position assigned by the plates, whereas, on the contrary, these shells may perhaps be in their normal state, as we find to be very frequently the case. It is thus evident, that it is not at all a matter of indifference to represent a shell in one way or another, and that it becomes indispensable for the zoologist, or the palæontologist, to afford geologists points of comparison upon which they may rely with certainty, to enable them to ascertain the condition of the strata, at the moment when the beings therein contained became covered by new deposits.

I have already remarked, that there is a great difference between the position of man, and that of ordinary quadrupeds. Now, we find another example of such a difference in the position of fishes having a symmetrical form, compared with that of the Pleuronectes; for the former are in a vertical position, whereas the others are, relatively to them, placed on the side. I insist on this last comparison of the position of fishes, because, in the bivalve acephala, we find precisely the same thing, as will appear from the following observations.

SYMMETRICAL SHELLS.

Whenever a bivalve shell is perfectly symmetrical in its parts, so that it is equivalve, we may assert *a priori*, that its position is vertical or nearly so, in the direction of the length.

The genera *Solen, Mya, Lutraria, Mycetopus, Panopæa*, &c., whose form is the most elongated, are examples. Generally they are much sunk either in the sand, or in the mud, where their tubes unceasingly perform an up and down movement, in order to arrive at the surface, and their position is perfectly perpendicular.

When the shell, equally elongated, pierces a hole in a stone, as we see in the genera *Pholas, Lithodomus, Saxicava, Clavagella, Teredo*, &c., the shell is also perpendicular, the tubes above, the mouth below.

When a free symmetrical shell is more or less oval or rounded, as is the case in the genera *Venus, Cardium, Tellina, Nucula, Pectunculus, Arca, Unio, Anodonta, Mactra, Donax, Cyclas*, it is still vertical, the tubes being above, and the mouth below; but sometimes it inclines a little to a side.

Symmetrical shells provided with a byssus which attaches them to the rock, have positions differing a little from one another. In the *Bisso-arca* and the *Venericardia*, they are fixed in such a manner as to give them the same attitude as the Venus in a free state. In the *Mytilus, Modiola*, and *Pinna*, the position is different, the hinge being placed below in place of being at the side, and the gaping portion of the valves being above. In this case, nevertheless, the animal is still in the same relative position, for the mouth is always below, and the anus above.

UNSYMMETRICAL SHELLS.

If, *a priori*, a symmetrical bivalve shell announces a vertical normal position in the direction of the large diameter, we are equally certain that all non-symmetrical bivalve shells have a natural position quite distinct, and analogous among the mollusca, to that of the Pleuronectes with respect to other fishes, that is to say, that the animal, in place of presenting its corresponding portions, or rather the line of separation of the two lobes of the mantle in a vertical direction, does so in a horizontal one; so that non-symmetrical bivalves are in the normal position, relatively to the others, when they repose on their sides. There is no longer in their case, as there is in all symmetrical shells, the distinction of *right valve* and *left valve*, but we have always an *upper valve* and a *lower valve*.

With the exception of the *Corbula* and *Pandora*, anomalous among the free shells, on account of their irregularity (although their normal position is vertical), all the other unsymmetrical bivalves are fixed either by means of a byssus, or by the shell itself.

When they are fixed by a byssus, they are much less irregular, as in the *Perna, Avicula, Malleus, Vulcella, Pecten*, &c., in which a scrupulous examination is sometimes requisite in order to discover the differences between one valve and the other.

When, on the contrary, the shell is fixed either to the ground or to submarine bodies by the calcareous matter of the shell itself, not only are the superior and inferior valves very unequal, but, moreover, these shells being constrained to conform in their growth to the space which has fallen to their lot, we find them either moulding themselves on the bodies on which they are parasites, or modifying themselves according to the conditions of existence in which they are placed, and thus producing so great an alteration in the form and aspect of different individuals of the same species, that it is necessary to forget altogether the ordinary limits of variations, and to allow a much wider range as to specific characters; and this is the case with the genera *Chama, Spondylus, Plicatula*, and especially with the *Ostrea* and *Gryphæa*.

In conclusion, I would sum up the subject by saying that the normal position of the acephalous mollusca is vertical, the tubes being above and the mouth below, in all symmetrical bivalves; whereas it is horizontal, the mouth on the one side and the anus on the other, in all unsymmetrical shells. In the first case we have a *right valve* and a *left valve*, and in the other an *upper valve*, and a *lower valve*. This normal position being the natural one to adopt, and capable of conferring great advantages on geological observations relative to the condition of the seas at different epochs and at different parts of a basin, I intend to retain it scrupulously in the representation of all shells; and, as it has been submitted to the test of numerous observations made in all latitudes, geologists may confidently rely on its accuracy, and compare the state of the faunas contained in the strata composing the crust of the earth.*

Report of the Researches of M. Agassiz during his last two sojourns at L'Hotel-des-Neuchatelois, upon the Lower Glacier of the Aar, in the years 1841 *and* 1842. By M. E. Desor.

The importance which the study of the glaciers has lately acquired, is so generally known, that but few words are necessary to recommend it to the attention of the public; and geology, in assigning to their influence so marked an agency in the resolutions of the globe, has suddenly raised them from the neglect in which they had long lain. The activity displayed in these investigations has been very much spontaneous, and almost universal; for there are few countries in which the new science has been put into requisition, in which the consequent observations have not led to the most satisfactory results. Recognising a development so extensive, I fear lest the researches, of which I purpose to give an account, will scarcely appear inviting, owing to the contracted locality to which they refer. Many, upon reading the title of this article, will, no doubt, enquire what so extraordinary is to be observed and studied in the glacier of the Aar; so that there was occasion to resort thither for two successive seasons, and remain for weeks and months together. To meet all prejudices of this kind, and enable the reader to judge for himself, I shall first, in a few words, give a view of the state of our knowledge at the close of the year

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* *Annales des Sciences Naturelles. Avril* 1843, p. 212.

1840, and beginning of 1841, the periods of the publication of the works of MM. Agassiz and Charpentier; and shall, at the same time, point out what were the problems which remained to be solved when M. Agassiz commenced the new series of observations upon which we are about to dwell.

The comparative study of the different regions of the glacier, and of the modifications which the ice undergoes in its course from the superior elevations towards the lower valleys, instantly occurs as one of the most important points. The older authors recognised its importance, and made a distinction between the glacier, properly so called, and the *Firn* or *Névé;* and more recent ones have recognised three distinct regions in all the larger glaciers, namely, the region of compact ice, the region of the Névé, and the region of snow-fields.*

Regarding the first region, or that of compact ice, it is known that the ice becomes harder and harder towards the extremity of the glacier. The presence of a net-work of very fine fissures —the capillary fissures—had also been ascertained, by which the water of the surface copiously penetrated into the interior; but these fissures were only known as they had been observed upon the surface, and upon the walls of the crevices. It was not positively ascertained if they extended to the interior of the mass, or to what precise depth they reached. Hence, among the objections made to the theory of infiltration, the argument has often been put forward, that previous to admitting a general infiltration by the capillary fissures, it was necessary to demonstrate the existence of these fissures throughout the mass. Many learned men and able natural philosophers, considered the idea as very problematical, and in opposition to the constituent nature of ice. It was, therefore, only direct experiments upon the infiltrations which could decide the question. As to the always increasing density of the ice, it was usually attributed to the effect of pressure, but without any accurate account being given of the nature of that pressure; and we were also in complete ignorance of the effects produced by the air-bubbles contained in the ice, and the modifications which they underwent. The crevices encountered in such numbers in the region of the compact ice were regarded as the effect of the tension of the mass; but we had only a most vague idea of the nature of this tension; and we shall see in the sequel, that even still there is great difficulty in explaining their functions. The ice tables, the gravelly cones, and the *baignoires*, were formerly explained in a satisfactory manner by the older Swiss naturalists, and among others by the MM. de Saussure, Kuhn, and the elder Studer. MM. Agassiz and Charpentier have added to this department a great number of new observations. Still, however, many additional ones were still required. The lamellar or ribbon structure in the middle portions of the glacier had been but vaguely observed, and no satisfactory explanation had yet been supplied. No more had the cause of those singular holes, similar in form, which are spread over all the surface of a glacier, and which we shall subsequently describe under the name of *meridian holes*, been discovered. Only an imperfect account had also been given of the different varieties which the ice of one and the same glacier frequently presents, upon a transverse section from one of its banks to the other. Finally, the stratification in ice, which is a very general phenomenon, had been only loosely described by some, and its existence had been altogether denied by others.

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* These regions have now for the first time been represented in a graphic manner, in a map which, along with M. Agassiz, I have prepared, as an accompaniment to the German translation of our ascent of the Jungfrau; the limits of the different regions being there indicated with all the precision which can be attained in a map which has not been constructed upon trigonometrical observations.

The Névé has been especially the object of the persevering study of modern observers, and the results of their investigations are worthy of the greatest attention. MM. Agassiz and Charpentier have both devoted a chapter to its consideration in their respective works. The latter of these gentlemen particularizes two kinds, namely, the higher and lower Névé. But, in spite of all this, much still remains to be done in this favourite department. How, for example, are the granules of the Névé formed? What series of transformations do these granules undergo in changing into ice, which becomes more and more compact, rendering this ice of the Névé more and more transparent? How does this variety of ice contain neither the capillary fissures, nor exhibit the blue bands? and how do they make their appearance all of a sudden? Finally, in virtue of what action is it that the transformation of the Névé into ice takes place from below upwards, instead of proceeding from above downwards? As to the limits of the Névé, it has been observed to be tolerably constant among the Alps; although this precision has evidently been exaggerated. Additional researches into this matter must still be undertaken.

The third region, which comprehends the snow-fields, is the least known of all. But few naturalists had studied these elevated regions, and those who had traversed them had not contributed much respecting the extraordinary phenomena which are there witnessed. It is without doubt known that it is there that the snows annually fall which feed and maintain the glaciers, and that the snow which falls during the winter upon the lower parts of the glaciers, melts during summer, without contributing, in a direct way, to the formation of the ice. But all our ideas concerning the history of the snow in these immense circuses have hitherto been very imperfect. We had not even a satisfactory knowledge of the form under which it habitually falls,—whether as flakes, or in fine powder, or as minute grains. No more had the particular form of the crevices in these elevated regions, which are different from those of the glaciers and the Névé, been elucidated. Finally, we were completely ignorant of what became of the horizontal and regular stratification, which is seen upon the sides of those abysses as deep as the eye can penetrate, and of which no trace is to be found in the crevices of the glaciers properly so called.

Theory was still more at a loss to explain the phenomena which are witnessed upon the highest summits, and the data we possessed concerning the state of the icy masses of these regions was still more contradictory. It was admitted that there the temperature was always at 32° F. or below it, and yet we talked of ice upon the highest peaks. Here there was evidently an exception to the common rule; and this ice formed above the fields of snow, implied particular conditions which had not hitherto been recognised. There was thus a strong motive to visit these elevated regions, and observe the temperature of the air, and the state of the ice.*

Temperature.—Previous to the time when M. Agassiz devoted himself to the study of the glaciers, absolutely nothing was known concerning the temperature of these icy masses. Nevertheless, it was very generally admitted that it must be very low. The first experiments were made in the year 1840, at the lower glacier of the Aar, where they indicated a temperature which was nearly constant, oscillating between 32 F. and 31°.5 F. But these observations having been made at a depth of only 25 feet, it might be feared that they were not completely independent of the external temperature. Hence, it became necessary to repeat them at different stations, and still more, at greater depths. It also became important to examine more in detail the relation of the external melting with the temperature in the shade and in the sunshine, to study the influence of nocturnal radiation, and to observe the action of the heat upon the water contained in the *baignoires* and the different cavities of the ice.

The phenomena connected with moraines are of primary importance in reference to the question of the transportation of boulders; hence these moraines have been studied in all their aspects; and their origin, formation, transport, their combinations, compositions, and different characters, have been described and figured with great care in the work of M. Agassiz; and we might have supposed that, in these points at least, the investigation was at an end. But new difficulties arose in connection with the researches concerning the method in which glaciers advance in their onward course. Intimate relations must of course exist between the form and arrangement of the moraines, and the more or less accelerated movement of the mighty mass. Hence arose the questions why certain median moraines at first narrow, as in the median moraine of the Aar, dilates in so remarkable a manner in the ultimate course of the glacier, although no new materials are added to it;—why certain lateral moraines entirely disappear in their course;—and by the influence of what cause it is, that moraines, after having formed very high ramparts, again become comparatively flat towards the extremity ot the glacier.

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* The year 1842 has been fertile in ascents, and thus the impulse given by the study of glaciers has greatly advanced the limits of our knowledge. The Finsteraarhorn has been scaled by M. Sulger, a citizen of Bâle, towards the end of the month of August. The Strahleck also has been frequently traversed. The Dent-du-Midi has been escaladed for the first time by seven natives of the Valais. The Jungfrau has again seen adventurous travellers risk their safety upon its snowy ridge: MM. Studer and Burky accomplished its ascent in August, conducted by five guides from the Grimsel, two of whom accompanied us the preceding year; and I have thus enjoyed the satisfaction of M. Studer's confirming in all respects the description I had previously published. Finally, I have myself scaled the Shreckhorn, along with my friend M. Escher of the Linth. An equally remarkable ascent has been accomplished in the Pyrenees. A Russian officer, M. de Tchihatcheff, has scaled the peak Nethon de la Maledetta; and it is interesting to remark that his observations, relative to ice and snow, correspond precisely to those we have made among the Alps.

The question of movement or progress is that which has always most engaged the attention of natural philosophers. And it is the one which most merits attention, because, whatever theory be adopted, we must always come back to movement before we can account for the existence of glaciers and their preservation, in those positions in which they could not have so formed themselves *in situ*. The displacements to which glaciers are subjected, are too considerable and frequent, even to be doubted by the native mountaineers, who regard this state of matters only as natural. In their estimation, the glacier is a river, which descends from the mountain, like its torrents, only in a manner somewhat different. In fact, such an intimate relation exists between a glacier and a river, that the strongest possible parallelism naturally suggests itself to the mind. A glacier is formed on the same principle, and is composed of the same elements, as a river; it is only water under another form, which moreover occupies, as do torrents, the bottom of valleys whose sinuosities it follows as if it were really liquid. All this was so plain, that those who first directed their attention to the subject, were at once convinced of it.

At the same time, when glaciers more particularly engaged the attention of philosophers, they suggested other theories; and opinions differed regarding the nature of their movement, and the causes which produced it. Scheuchzer was the first who, admitting the slow and gradual movement, ascribed it to the infiltration and congelation of the water contained in the capillary fissures. Gruner believed the movement less regular, and that it was determined by the weight of the mass, and by the melting of its sides. De Saussure very much adopted the same view. "These icy masses," he remarks, "descending the slope of the bed on which they rest, released by the streams from the hold they might take upon this same bed, sometimes even uplifted by this water, must gradually glide and descend in the course of the slope of the valleys, and of the ridges which they cover." Such, too, is nearly the opinion of Kuhn that observer who, of all others during the last century, best comprehended and described the whole phenomena of glaciers. One thing is very clear, that no one in Switzerland ever doubted that the glaciers were subject to movement. And notwithstanding, the fact in itself is not so simple as it appears to those of us who are familiar with it as a matter of public notoriety. It is thus highly interesting to read over the controversies which the inquiry occasioned towards the close of the last century. One of the Professors of the University of Tubingen, M. Plouquet, undertook an excursion into Switzerland, for the purpose of studying the subject of the glaciers. When he beheld these immense masses, covering whole leagues, and occupying all the sinuosities of the valleys, he could not imagine that they were subjected to any movement whatever; and he remarked that, if possessed of a gliding motion, they must long before have invaded the plains and valleys into which they debouched. Hence this author thought that the glaciers were immovable, and that if a movement were possible, that movement must needs be very slow and gradual; but as such a motion appeared to him in the highest degree unlikely, he assumed that glaciers in general are stationary, and have ever been so. This opinion, published in the year 1786 under the title of *Vertrauliche Erzæhlung einer Schweizer Reise*, roused noisy clamours among the Swiss *savans*, who regarded it as a decided heresy. *Höpfner's Magazine*, a distinguished scientific journal which appeared at this time at Zurich, headed the defence; and M. Kuhn undertook, in one of its numbers, to refute the opinion of the German philosopher, afresh substituting in its place the popular and prevailing belief. He adduced facts—that at such a time a particular field or meadow was invaded, trees were uprooted, and huts overturned; and, as if these glaciers were predestined to induce irritating discussions, we find that even then the controversy assumed a very keen and angry tone. M. Plouquet answered M. Kuhn's articles in a second memoir, in which he explained anew, and at more length, the views and reasons which opposed his admitting that any, and especially a rapid movement, took place; and retreating upon his scepticism, he demurred, as inadmissible, to all proofs borrowed from the vague notions of the mountaineers. He demanded precise facts, and especially measurements; and, as few of the *savans* had themselves made observations, and mistrust now began to be entertained regarding the recitals of the natives, all this made for the cause of M. Plouquet. But De Saussure and Kuhn both had ocular demonstration of the motion. The following is the statement of the former. "In the month of July 1761, along with my guide, Pierre Simon, I passed under a very high glacier which is to the west of that of the *Pèlerins*. There I observed an immense block of granite, cubical in form, and more than 40 feet long in all its dimensions, resting upon the debris at the foot of the glacier, and deposited in its place by this glacier. Let us be quick, said Pierre Simon to me; for the ice which presses upon the rock will soon force it forward, and so overwhelm us. Scarcely had we safely passed it, when it began to move; at first it slid almost imperceptibly upon the debris lying under it; it then tumbled over upon its anterior face, then upon another; it then began to roll over, and the declivity increasing, it fell from spot to spot, till it began to bound away, first to a small extent, and then to great distances; at each bound it shivered away splinters, then great blocks, and finally the rocks on which it fell; all these fragments followed its wake down the slope of the mountain, and it thus formed a torrent of rocks, great and small, which overwhelmed a forest in which it was finally arrested, after having cleared a way for itself, in a few moments, to the extent of half a league, with a noise and destruction altogether astonishing." (De Saussure, Voyages dans les Alpes, tome i., p. 384, § 538.)

M. Kuhn observed a still more striking movement, occasioned by the formation of a crevice in the glacier of Grindelwald; and hence he concluded that this was the cause of the advancing motion. It could not then be denied that glaciers possessed a movement, which was continued from day to day; a circumstance which was not afterwards sufficiently attended to. M. Venetz still maintains the theory of gliding in his celebrated memoir upon the variations of temperature in the Alps; where he narrated that there were glaciers which made sudden bounds forwards to the extent of ten feet. Here, however, it should be noted, that this phenomenon was not observed by M. Venetz himself, and that the statement rested upon the simple assertion of the native mountaineers. Add to this, that since attention has latterly more steadily been directed to the subject, no example of this kind of advance has been recorded. Nothing then remains, but that we should return to the slower movement.

M. Toussaint de Charpentier, brother of the celebrated geologist of Bex, and M. Biselx, prior of the Convent of St Bernard, revived the theory of dilatation; but from this period the question became in a wonderful degree complicated. It now became necessary to explain what the influences were by which the movement was effected, and whether it was continuous, or occurred only at certain times? and, under the last alternative, what were the periods of progression? also, if the movement was uniform, or, on the contrary, was liable to times of retardation and acceleration? These questions, it was evident, could not be determined in a manner at all satisfactory, unless by accurate measurements. When we consider the paucity of the measurements which we possess even now, we may well be astonished that any theory whatever could be based upon such meagre data. The admeasurements commenced by M. Hugi upon the glacier of the

Aar, and continued at a later period by M. Agassiz, are in fact the whole of the materials of this kind which we possessed. It remained, therefore, not only that we should continue these annual measurements, but, that they might be invested with a real value, it was necessary they should be multiplied in numerous places. Daily observations were necessary to appreciate the diversity of the movement in hot or humid days, and during cold ones; and it required that these observations should be repeated many times every day, that the difference between the diurnal and nocturnal movement might be brought out. Finally, that we might ascertain the movement from one season to another, it was indispensable to determine trigonometrically numerous points over the whole extent of the glacier, which might be measured at all seasons of the year. In a word, it became necessary to prepare a chart of the whole glacier.

With this question of the motion of the glacier, is connected the one respecting the changes which the sarface of the glacier undergoes. It is evident, that if the loss of substance which the glacier undergoes is compensated only by the advancement of the superior masses, there ought to be a certain relation between the superficial waste and the progression. But before we can arrive at precise results on this head, we must also make a series of observations in different positions where the respective levellings are accurately taken, and that these observations must be continued for a certain time, so that the influence of external agents may be fully appreciated.

But in what way soever a glacier may move, it is a necessary condition of this movement that it shall free itself from the surface upon which it rests. This, at first view, at all events, appears a condition imperatively required; although there seem to be facts which go to prove that the glacier is frozen at its lower surface, and MM. Agassiz and de Charpentier are disposed to admit there may be a slow movement, even supposing that the lower surface of the ice was frozen to the part beneath. However this may be, this theory requires to be supported upon additional observations before it can be seriously entertained or examined; and we cannot hope to arrive at any positive conclusions, except by sounding the glacier, or at least placing ourselves in circumstances where we may continually observe it.

Finally, the whole of the theory, whether in relation to physics or geology, had to endure attacks from a variety of quarters. Infiltration was strictly denied by some, and many were opposed to the notion that the glacier was frozen to the parts beneath; and admitting the facts which appeared contrary to this opinion, the theory of gliding must again be canvassed.

The phenomena of glaciers, in connection with their change of position, also become the subject of various attacks. Some had endeavoured to restrict their influence within very narrow limits; and some philosophers maintained the idea, that the glaciers, at no epoch, had advanced more than half a league beyond their present limits. Others allowed that at one time they must have reached the issues of the great valleys; whilst others prolonged their course as far as the Jura. On the other hand, there were a number of geologists who adopted the opinion of M. Agassiz as the most probable, and thought that they had discovered erratic phenomena in many localities and countries where the presence of glaciers had never before been dreamt of.

The theory of currents even, was subjected to many changes, especially as regarded the origin of the pretended great ones. The time was when it was with much simplicity supposed that an impulsion,—some violent shock,—occurred at the summit of the Alps, and had forced from the spot, with unheard-of velocity, enormous debris of rocks. It was long before a cause for these currents was thought of. First, it was conceived that the district of the Valais had been an immense lake, whose waters had overwhelmed the plain in consequence of a cataclysm which separated the Dent du Midi from the Dent de Morcles. Others, again, assuming as a type the *debacle* of the valley of Bagnes, fancied that many lakes were formerly barred by glaciers,—that when these maintaining barriers came to be broken, currents were produced which transported immense boulders to the opposite slopes of the Jura. There were others, again, who entirely denied the alliance supposed to exist among some of the phenomena which had been associated in the theory of glaciers, and especially as regarded this polish upon rocks, and the various striæ and grooves which it was alleged had been produced by their enormous pressure.

All these different opinions, interpreting so variously the phenomena which had been accumulated, made the acquisition of new and more detailed observations altogether indispensable: and it was important that these should be made as much as possible upon the glaciers now existing; in fact, that their current operations should actually be watched. Besides, the observation we had made the previous year concerning a constant limit of the polished rocks, was of such paramount interest that it merited peculiar attention.

Thus the immensity of the field was evident. And however important may have been the results of the researches of late years, much still remains to be accomplished; especially when we consider that here, as in every new science, the solution of one problem only gives rise to the consideration of another, and that what appeared at one time sufficiently established, is presently involved in doubt by discovery of new facts which may oblige the observer to undertake a new series of investigations. If innumerable difficulties which are met with in conducting researches, apparently simple, cause our progress to be slow, it is no less true that the slightest observation, and a single fact needing confirmation, require long and arduous journeys; and withal, nature in these elevated regions is so capricious, that we must almost steal her favours that we may profit by the slightest circumstances of time and place.

We have no intention of entering into the details of all the observations which were made at the *Hôtel des Neuchatelois* during the last two years. These will appear in a separate volume which M. Agassiz is now preparing as a sequel to his *Etudes sur les Glaciers*, and of which notice will subsequently be taken. The object of this article is only to exhibit, in a summary manner, the sojourn of M. Agassiz and his companions at the lower glacier of the Aar, pointing out the principal results they have obtained. Instead of a journal or succinct narration of events in their chronological order, I shall treat separately of their principal operations, uniting under the same head the observations of both years, with the indication of their dates. One part of the results obtained during the summer of 1841 has already been published by M. Agassiz in the Edinburgh New Philosophical Journal (vol. xxxiii. p. 217); and some of them of 1842 will be found in the *Comptes Rendus* of *L'Institut*, and in Leonhard and Bronn's Journal.*

(*To be continued.*)

Meteorological Tables for 1843.

(Continued from Vol. XXXIV., p. 372.)

TABLE III.—MARCH.

1843.	Ther. Max.	Ther. Min.	Ther. Med.	Barom. ½ p. 8 A.M.	Therm. ½ p. 8 A.M.	Bar. 8 P.M.	Ther. 8 P.M	Rain.	Hail.	Snow.	Winds.	Meteors.
Mar. 1.	36°	27°	31°	29.68	31°	29.81	21′	...	...	...	N.	
... 2.	36	27	31	29.92	33	30.01	33	...	...	...	N.	
... 3.	38	24	31	30.04	27	30.15	32	...	...	..	N.	
... 4.	40	32	36	30.20	29	30.17	35	...	...	...	W.	
... 5.	45	38	41	30.09	36	30.00	40	...	...		W.	
... 6.	46	40	43	29.98	40	30.00	43	...	...	...	W.	
... 7.	47	28	37	30.04	41	30.07	37	...	...	...	S. E	
... 8.	41	30	35	30.19	29	30.20	35	...	..	...	S. E.	
... 9.	44	37	40	30.20	33	30.10	41	...	...	...	S. W.	
... 10.	43	40	41	29.88	38	29.88	41	...	...	...	N.	
... 11.	48	42	45	29.99	44	29.87	43	...	...	...	W.	
... 12.	46	35	40	29.58	44	29.36	40	...	...	...	W.	
... 13.	42	30	36	29.31	38	29.27	38	...	...	...	W.	
... 14.	43	30	36	29 24	34	29.34	38	...	...	...	W.	
... 15.	46	33	39	29.60	34	29.80	39	...	...	...	N. E.	
... 16.	45	40	42	29.88	35	29.65	41	...	...	...	S. W.	
... 17.	53	41	47	29.56	44	29.59	44	...	...	...	S. W.	
... 18.	54	40	47	29.71	46	29.86	41	...	...	...	S. E.	
... 19.	48	38	43	29.82	44	29.83	43	...	...	...	S. E.	
... 20.	44	39	41	20.71	41	29.51	40	...	...	...	E.	
... 21.	54	42	48	29.31	44	29.29	46	...	...	...	S. E.	Lightning.
... 22.	60	46	53	29.19	47	29.30	49	...	...	...	S.	
... 23.	55	40	47	29.24	47	29.46	47	...	...	...	S.	
... 24.	45	40	42	29.62	43	29.75	42	...	...	...	E.	
... 25.	44	36	40	29.92	43	30 02	37	...	...	...	E.	
... 26.	45	31	38	30.05	39	29.98	34	...	...	...	E.	
... 27.	38	33	35	29.90	35	29.88	34	...	...	...	E.	
... 28.	42	31	36	29.85	38	29.86	35	...	...	...	E.	
... 29.	44	30	37	29.91	37	29.87	38	...	...	...	E.	
... 30.	46	45	45	29.64	35	29 21	45	...	...	...	S. E.	
... 31.	59	44	51	29.08	39	29.16	48	...	...	...	S. W.	
Means.	45.70	35.77	40.45	29.752	38.64	29.750	39.67	15	4	2		

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* *Bibliothèque Universelle de Genève. April* 1843.

RESULTS.

BAROMETER.		THERMOMETER.	
Highest, . . .	30.20	Highest, . . .	60°
Lowest, . . .	29.08	Lowest, . . .	24°
Mean, . . .	29.751	Mean, . . .	40°.45

WINDS.

W. 7; N.W. 0; N. 4; N.E. 1; E. 7; S.E. 6; S. 2; S.W. 4.

NOTES.—*March* 1. Fine. 2. Fine; cloudy. 3, 4. Fine. 5. Fine; windy. 6. Windy. 7. Fine. 8. Foggy. 9. Cloudy. 10. Rainy. 11. Fine; cloudy. 12. High wind; cloudy, A.M. 13, 14. Rain and hail in showers. 15. Rain; cloudy. 16. Rainy; thermometer at 9 P.M. 46.° 17. 18. Fine; foggy. 19. 20. Cloudy. 21. Close, and cloudy A.M.; at 5½ thunder and heavy rain. 22. Rainy A.M.; fine. 24. Rainy and windy. 25. Windy. 26. Windy; gale with snow at night. 27. Cloudy. 28. Fine. 29. Fine. 30. Hazy; rainy and windy. 31. High wind; cloudy.

MEMORANDA.—*Earthquake at Lochgilphead.*—A smart shock of an earthquake was felt at Lochgilphead at forty minutes past 8 P.M. on Saturday last. It lasted from thirty to forty seconds, and was so strong, that the dishes on the wall and shelves were set in motion. The shock was accompanied by a loud noise, as of many carts of stones being emptied on the street. Many of the inhabitants ran in great alarm to their doors to ascertain the cause of the disturbance. A farmer who resides two miles and a half from Lochgilphead, mentions that the same sensation was felt at his house as at the village.

OBAN.—On Saturday evening the 25th, a shock of earthquake was distinctly felt in this place about 8 P.M. It appeared to pass from E. to W., and occupied from forty to fifty seconds in its transition. A flash of lightning was observed about the same time. About two years ago a similar shock was felt in this place, and a still severer one fourteen years since.

FLOODS AT ROME.—Letters from Rome of the 8th, give frightful accounts of destruction caused by the overflowing of the Tiber. The river had laid one-third of the Eternal City under water, forming deep lakes in many localities. The principal street, the Corso, the approaches to the castle of St Angelo, and the Piazza di Spagna, were completely flooded. The flood had continued for two days, and there was no appearance of its abatement, &c.

EARTHQUAKE IN THE WEST INDIES.

Copy of a Letter from R. J. Fayers, R.N., to his Excellency the Governor of Bermuda, concerning the Earthquake in the West Indies.

ROYAL MAIL STEAM-SHIP, FORTH,
14th Feb. 1843.—*Off Bermuda.*

SIR,—I think it expedient you should be in possession of circumstances that have recently occurred, and the intelligence conveyed to you as correctly as possible.

An earthquake (I fear of great extent) has occurred. On the 8th of February, at half-past 10 A. M., I was sitting at table with Mr Comrie, of the Colonial Bank at St Thomas, his private house being on a declivity half way up the highest part of the town. We were suddenly surprised by a confused sound, very much resembling the action of a strong draught of one of the large steamers' flues, with the door shut, attended by a hissing sound. Our first impulse was to fly to the balcony; every thing was on the move—windows rattling, and plaster falling off.

The streets immediately under presented an extraordinary scene, people rushing out of their houses, and crying in all directions, many fainting. The shock came not perpendicular, but horizontal, then a perfect calm. All business was suspended, and churches were opened for worship. The ships at anchor in the harbour felt the shock very plainly, and those who were below, at the time came on deck to ascertain the cause of the disturbance.

A French brig coming to St Thomas's, felt the shock so severely, whilst off Tortola, that he thought the vessel had struck on a rock. The Spanish man-of-war corvett Cubana, came in from the westward, and though close in to the west end of St Thomas', did not feel any shock. A vessel arrived from St Kitts, the Court-House and Custom-House had fallen, but no mention of any loss of life. On the following day, at 8 A. M., the royal mail steamer "Thames" arrived; the news brought by her was, that, to the great dismay of all on board, the Thames was brought up as if on a reef of rocks. The captain, Philip Haste, had his attention called, by a passenger exclaiming, Heavens, look at the land! All was enveloped in a cloud of dust. The shock had passed Montserrat, which continued, as the Thames passed, enveloped in a cloud of dust. God only knows the fate of the islands to the eastward. I have the honour to be, your Excellency's obedient servant.

Captain Haste's Letter is as follows —

SIR,—I lament to inform you that we have experienced several terrific shocks of earthquake. The effect on all hands was indescribable, all came running up from below, not a man was left in the engine-room, except Mr Casey, the chief engineer, who insisted that something had gone wrong with the boilers.

On looking at the island an awful scene presented itself, clouds of dust rising in every direction, &c. Montserrat, although thirty miles distant, we could see was enveloped in dust; some severe shocks were felt in this island at the same time. The weather during the earthquake was beautiful.

Moderate breeze east; barometer, 30.15; thermometer, 81.°

Falmouth, 7th March.—From a passenger who returned in the Actæon from the West Indies, we have the following:—

No language which I could employ, would be adequate to convey to the minds of others the idea of the nature of the catastrophe, or of the destruction, desolation, and misery which have followed in its wake. On the morning of the 8th February, the greatest consternation and terror prevailed amongst the inhabitants. St Eustatius, in St Kitts, suffered severely.

Letter from a Gentleman who was in the island at the time.

There was nothing very remarkable in the atmosphere on the morning of the 8th. The sky was clear, and the wind fresh and rather cold. There was, however, a deep purple haze in the air, rather uncommon at this time of year. About twenty-three minutes past eleven, I went to the quarter-deck of her Majesty's steamer "Dee;" every one was busily employed, when suddenly I saw the cliff behind the coal-yard undulate. On turning my eyes up the harbour, I saw a hill, called Monk's Hill, toppling from its summit, and enveloped in dust. The water in the harbour foamed and bubbled, and in many places a white substance rose, as if thrown up from the bottom. After the shock had passed, to my dying day I shall never forget the horror that was depicted in every countenance. Men gazed at each other in blank and terrible dismay, &c.

Besides the immense destruction of buildings, it has been ascertained that the lives of two thousand persons have been sacrificed, and from one thousand two hundred to one thousand five hundred individuals maimed and mutilated.

TABLE IV.—APRIL.

1843.	Ther. Max.	Ther. Min.	Ther. Mean.	Barom. ½ p. 8 A.M.	Therm. ½ p. 8 A.M	Bar. 8 P.M.	Ther. 8 P.M.	Rain.	Hail.	...	Wind.	Meteors.
April 1.	55°	44°	49°	29.16	49°	29.02	50°	...	...	...	S. W.	
... 2.	55	44	49	29.18	49	29.22	45	...	...	...	S. W.	
... 3.	54	42	48	29.43	48	29.50	45	...	...	...	W.	
... 4.	52	40	46	29.30	47	29.21	45	...	...	...	S.	
... 5.	52	38	45	29.39	43	29.57	44	...	...	...	N.	Aurora Bor.
... 6.	53	44	48	29 40	45	29.28	44	...	...	...	W.	Aurora Bor.
... 7.	53	38	45	29.19	47	29.06	44	...	...	...	W.	
... 8.	45	34	39	29.07	42	29.48	39	...	...	...	E.	
... 9.	46	31	38	29.61	37	29.69	36	...	...	...	N.	
... 10.	42	28	35	29.73	36	29.80	35	...	...	...	N.	Lunar Halo.
... 11.	42	27	34	29.86	36	29.86	34	...	...	...	N.	
... 12.	44	30	37	29.80	35	29.64	34	...	...	...	W.	
... 13.	42	35	38	29.80	36	29.77	35	...	...	...	N.W.	
... 14.	54	43	48	29.45	48	29.68	48	...	...	...	W.	
... 15.	53	45	49	29.80	49	29.83	47	...	...	...	W.	
... 16.	55	38	46	29.78	51	29.78	48	...	...	...	W.	
... 17.	60	46	53	29.85	46	29.89	50	...	...	...	S.	
... 18.	58	45	51	29.90	50	29.81	50	...	...	...	S. W.	
... 19.	64	41	52	29.80	52	29.76	50	...	...	...	S. W.	
... 20.	56	45	50	29.15	49	29.57	47	...	...	...	E.	
... 21.	59	48	53	29.64	51	29.67	50	...	...	...	S. W.	
... 22.	51	40	45	29.61	50	29.69	45	...	...	...	Var.	
... 23.	48	37	42	29.73	43	29.80	43	...	...	...	Var.	Aurora Bor.
... 24.	58	40	49	29.89	45	29.80	49	...	...	...	S. W.	
... 25.	52	33	42	29.29	42	29.31	42	...	...	...	N. W.	
... 26.	50	32	41	29.32	42	29.51	40	...	...	...	N. E.	
... 27.	55	43	49	29.69	40	29.62	48	...	...	...	S. W.	
... 28.	50	37	43	29.50	45	29.47	43	...	...	...	S. W.	
... 29.	56	43	49	29.50	44	29.80	45	...	...	...	S. E.	
... 30	50	42	46	30.07	46	30.19	43	...	...	...	E.	
Means,	53.13	39.10	45.30	29.563	44.76	29.609	43.93	18	2	4		

RESULTS.

BAROMETER.		THERMOMETER.	
Highest,	30.19	Highest,	64°
Lowest,	29.02	Lowest,	27°
Mean,	29.586	Mean,	45°30

W. 7; N.W. 2; N. 4; N.E. 1; E. 3; S.E. 1; S. 2; S.W. 8; Var. 2.

At Kinfauns (for last month) the extremes of pressure were 30.35 and 29.20; temperature 52° and 16°; mean temperature 39°.67; amount of rain 1.35 inch.

NOTES.—*April* 1, 2. Showery and windy. 3. Cloudy. 4. Rainy. 5. Rainy; aurora remarkably bright this evening, although moonlight. Almost immediately below the moon there was a large mass of luminous matter, through which the stars were distinctly visible. 6, 7. Windy. 8. Rainy. 9. Fine. 10, 11. Snow and hail in showers. 12. High wind, rain; at half-past 4 P.M. heavy snow and windy. 13. Cloudy, windy, and snow at 9 P.M.; high wind and rain during night. 14. High wind. 15, 16. Cloudy. 17. 18. 19. Fine. 20. Cloudy. 22. Rainy.

23. Rainy; fine. 24. Cloudy. 25. Stormy, A.M.; fine. 27. Cloudy night; rainy. 28. Rainy. 29. Fine; foggy P.M.

MEMORANDA.—*Earthquake in Holland.*—ZEALAND, 6*th April.*—This morning, about two o'clock, a slight shock of earthquake was felt here. About half-past six, it was followed by a second and still more severe shock. The church bell was so violently agitated, that the ringing was heard at a considerable distance. Accounts from Uden mention that the shock was also felt there, and that the people who were assembled in the church hastily ran out. Houses were shaken violently, articles of furniture were removed from their places, and persons in bed were thrown from one place to another.

BOIS LE DUC, 9*th April.*—The dike of the Zuid Willemswaart canal has given way, and sunk to the extent of twenty yards. Letters received to-day from St Oderode, mention that several shocks had been felt there on the 7th (Friday), about 11 P.M. The inhabitants rushed out of their houses for fear of being buried under the ruins, and remained in the streets until daylight returned.

Several localities in North Brabant have been visited by the earthquake. Grave, Bommel, Husnew, Breda, Telburgh, Findhoven, and other places, have received some severe shocks. At the same time there occurred a storm of thunder and lightning. At Gorcum a very heavy shock was felt at a quarter before six A.M.

TABLE V.—MAY

1843.	Ther. Mox.	Ther. Max.	Ther. Min.	Barom. ½ p. 8, A.M.	Therm. ½ p. 8, A.M.	Bar. 8, P. M.	Ther. 8, P. M.	Rain.	Hail.	—	Wind.	Meteors.
May 1.	57°	38°	47°	30.29	50	30.29	46°	...	...	...	E.	
... 2.	53	39	46	30.22	47	30.09	43	...	...	...	E.	
... 3.	60	40	50	29.90	42	29.69	46	...	...	...	E.	
... 4.	57	38	47	29.55	50	29.41	47	...	...	...	W.	
... 5.	53	44	48	29.33	45	29.30	47	...	...	...	S. W.	
... 6	56	42	49	29.39	47	29.34	47	...	...	...	N.	
... 7.	57	40	48	29.29	48	29.48	44	...	...	...	W.	
... 8.	53	43	48	29.63	46	29.76	44	...	...	...	E.	
... 9.	51	41	46	29.81	46	29.92	45	...	...	...	E.	
... 10.	50	38	44	30.07	46	30.10	41	...	...	...	E.	
... 11.	53	41	47	30.10	44	30.02	45	...	...	...	E.	
.. 12.	57	48	52	30.00	48	29.75	50	...	...	...	E.	
... 13.	58	46	52	29.59	54	29 60	51	...	...	...	S. W.	
... 14.	63	46	54	29.65	51	29.58	48	...	...	...	W. E.	
... 15.	56	43	49	29.51	48	29.51	46	...	...	...	E.	
... 16.	47	44	45	29.53	46	29.58	44	...	...	...	N. E.	
... 17.	51	40	45	29.69	48	29.83	44	...	...	...	E.	
... 18.	50	40	45	29.91	47	29.97	45	...	...	...	E.	
... 19.	54	41	47	29.99	47	29.91	44	...	...	...	E. S. E.	
... 20.	54	42	48	29.88	47	29.80	45	...	...	...	S. E.	
... 21.	47	42	44	29.71	46	29.70	44	...	...	...	E.	
... 22	46	42	44	29.70	45	29.69	44	...	...	...	E.	
... 23.	48	43	45	29.70	47	29.71	46	...	...	...	E.	
... 24.	49	43	46	29.69	47	29.63	45	...	...	...	E.	
... 25.	52	46	49	29.40	47	29.38	49	...	...	...	W.	
... 26.	63	46	54	29.37	54	29.30	52	...	...	...	S. W.	
... 27.	60	44	52	29.28	52	29.27	50	...	...	...	W. N.	
... 28.	51	37	44	29.50	48	29.70	44	...	...	...	N. E.	
... 29.	52	38	45	29.78	46	29.85	44	...	...	...	N.	
... 30.	58	43	50	29.90	46	29.82	47	...	...	...	N. S. W.	
... 31.	45	43	44	29.68	44	29.61	44	...	...	...	E.	
Means,	53.90	41.96	47.54	29.710	47.38	29.696	45.83	21	4			

RESULTS.

BAROMETER.		THERMOMETER.	
Highest,	30.29	Highest,	63°
Lowest,	27.27	Lowest,	37°
Mean,	29.703	Mean,	47.54

WINDS.

W. 4; N.W. 0; N. 3; N.E. 2; E. 17; S.E 1½; S. 0; S.W. 3½.

Notes.—*May* 1.—3. Hazy. 4. Cloudy; rain and hail P.M. 5. High wind; showery. 6. Cloudy. 7. Rainy P.M. 8. Cloudy; windy. 9–11. Fine. 12. Cloudy; rainy. 13. Cloudy; windy. 14. Fine; foggy. 15. Cloudy; windy. 16. Rainy and windy; hazy. 17. Cloudy; hail 11 P.M. 18–20. Fine. 21. Rain P.M.; windy. 22. 23. Hazy and rainy. 24. Cloudy; during night windy and rainy. 25. Cloudy. 27. Evening rainy; misty; windy. 28. Cloudy; hail P.M.; rainy. 29. Showery; windy. 30. Fine; rainy night. 31. Rainy; hazy. The Pentland Hills were covered with snow on the morning of the 29th of May.

Curious Fact.—For several days the artesian well of Grenelle has thrown up small black fishes, which have no apparent eyes. This phenomenon was observed last year at the same period. The Academy of Sciences has ordered a report to be made on that extraordinary fact.—*Edinburgh Advertiser*, May 30.

The late Earthquake.—A letter from an inhabitant of note of Guadaloupe, dated 7th March, giving an account of a phenomenon which appears to have been connected with the catastrophe of the 8th February, has been communicated to the Academy of Sciences by the Minister of War. M. Celeron, the gentleman in question, relates, that between the eastern point of Marie Galante and Guadaloupe, and in the mid channel, a column of water, black in colour, and of large diameter, rose from the sea. This appearance lasted about half an hour. M. Celoron adds, that, being well acquainted with the nature of waterspouts, he was certain that this was not one, as the column was too vertical, and had no communication with the clouds. No doubt was entertained by him of its being the effect of a submarine volcano.

Second Earthquake in the West Indies.

Charleston, *22d March.*—We learn from Captain Smith of the schooner Francis Cannaday, that a shock of earthquake was experienced in the northern part of Guadaloupe on the 3d (March). At the time a captain of a vessel, off the north point of the island, mentioned, that it shook the vessel with such severity, so that it was with difficulty the crew could keep their feet. A dense cloud of smoke ascended from the Bassaterre, and serious fears were entertained for the safety of that place. At

Point Petre the atmosphere was so impregnated with the effluvia arising from the ruins of the town, as to be quite unsupportable. The comet recently seen at this place was observed at St Thomas's on the 2d April, and was so brilliant, as to cause considerable alarm to the inhabitants. A shock of earthquake was also felt at St Thomas's on the 5th instant, about half-past nine at night. No material damage was done.

On the Generation of the Polygastric Infusoria. By Professor Owen, F.R.S., &c.*

Perhaps the most marvellous part of the organization and economy of the Polygastric Infusoria is that which relates to the function of generation. This function, I may observe, is the only one which does not necessarily require a special organ for its performance. I am not aware that this proposition has been before enunciated, but it will be quite intelligible when the essential nature of the generative process is better understood.

Although both ovaria and testes have been unequivocally demonstrated in the *Polygastria*, yet their most common mode of propagation is quite independent of, and superadded to, the function of these organs. In a well fed *Monas, Leucophrys, Enchelys, or Paramecium*, the globular parenchyme may be observed to become a little more opaque and apparently more minutely subdivided: then a clear line may be discerned, stretching itself transversely across the middle of the body and indicating a separation of the contents into two distinct parts. The containing integument next begins to contract along this line, and the creature to assume the form of an hour-glass. This, though doubtless an uncontrollable, seems to be a spontaneous action, and the struggle of each division to separate itself from its fellow indicates an impulse in each to assume its individual and independent character; the which they no sooner effect than they dart off in opposite directions, and rapidly acquire the normal size and figure. In the *Vorticella* and some other species, we have examples of spontaneous division in the longitudinal direction, which commences at the mouth, and extends to the irritable and contractile stem, from which one or both of the new formed individuals detach themselves. In some species, this spontaneous fission, which corresponds, as I stated in my Lectures on Generation in reference to the ova of the *Medusa*, in so interesting a manner with the earliest phenomenon in the development of the ovum in the higher animals, is arrested before its completion, but the partially separated individuals continue in organic connection, and form compound animals, sometimes in the form of long chains, sometimes branched, sometimes expanding to form a spherical bag, as in the well-known *Volvox globator*, which was long deemed a single individual of a peculiar species. New spherical groups of *Volvoces* are thrown off into the interior of the parent monadiary, which is rent open to allow them to escape.

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* From the Hunterian Lectures on Comparative Anatomy, delivered at the Royal College of Surgeons, London, in 1843, p. 22, &c.

Another mode of generation is by gemmation or the development of buds, which in some species, as *Cheroma*, grow out of the fore part of the body, and in others, as *Vorticella*, from the hind part near the stem, or from the stem itself, from which the young animal soon detaches itself. In most *Vorticellidæ*, as in *Carchesium* and *Epistylis*, the small liberated end of the body opposite the mouth is provided with a circle of vibratile cilia, so long as the individual swims freely; but these disappear when the pedicle is developed.

With regard to the more common fissiparous mode, Ehrenberg has figured gradations of this spontaneous division of the organised contents of the integument in the *Gonium* and *Chlamydomonas*, which may be compared with the earliest stages of the development of the germ, as figured by Siebold in the *Strongylus* and *Medusa*, by Baer in the frog, and by Barry in the rabbit. Dr Martin Barry, who has discovered the very remarkable and complicated nature of this process in the mammalian ovum, was alone perhaps in the condition to fully comprehend and explain its analogy to the fissiparous generation of the Polygastria, to which, in 1840, I briefly alluded; and this he has done in a paper, replete with interesting generalizations, lately read before the Royal Society. I have been favoured by that indefatigable observer with the following notes of his ideas on this subject.

"Between the appearance presented by the mammiferous germ during the passage of the ovum through the Fallopian tube, and those met with in the young Volvox globater while within the parent, I find a resemblance which is very remarkable indeed, extending even to minute details. Not only do the cells of which the young Volvox is composed form a body resembling a mulberry, with a pellucid centre, but the cells gradually increase in number, apparently by doubling, at the same time diminishing in size, like the cells of the mammiferous germ; which they resemble also in being originally elliptical and flat.

"Some of the points of resemblance now mentioned were recognised in the delineation of the Volvox given by professor Ehrenberg; others were noticed during some observations I have myself made on this very interesting microscopic object. Professor Ehrenberg has figured five pellucid globules in a young Volvox just escaped from the parent. These, the germs of another set, evidently resulted from division of the pellucid mass visible in another state: so that here is to be recognised fissiparous generation of the kind I have described as reproducing cells.

"On examining the figures given by Ehrenberg of successive generations of the Chlamydomonas, I see a resemblance to the two, four, eight, &c. groups of cells in the mammiferous ovum too striking, not to suggest that the process of formation must be the same in both: the essential part of this process consisting in division of the pellucid nucleus. And it is deserving of remark, that Ehrenberg describes his *Monas bicolor* evidently a nucleated cell, as possibly an early state of the *Chlamydomonas.*

"The curiously symmetrical forms of many of the *Bacillaria* appear to be due to this two, four, eight, &c. division of the nuclei of cells.

"The delineations of *Gonium*, *Monas vivipara*, and *Ophrydium* given by the great naturalist just mentioned, afford most satisfactory examples of a pellucid globule, dividing and subdividing like the hyaline in cells.

"In many other of Ehrenberg's figures of the Polygastric Infusoria, the corresponding part appears to me to be denoted by a blue, red, or green colour, according as there had been added either indigo, carmine. or sap-green. This accords with what has been mentioned in a former page, regarding cells, namely, that a foreign substance becomes added and assimilated through the hyaline.

"Fecundation of the ovum takes place in the same manner as nutrition of the cell, and seems, in some instances at least, comparable to the nutrition of one of the Infusoria.

"But farther, I recognise in Ehrenberg's delineations of the Infusoria, not merely a cell-formation, but everywhere the existence of transitory or assimilative cells.

"And farther still: the infusorial cells, like the cells of the larger organisms, have their origin in globules which become discs or 'cytoblasts;' these passing through stages such as those of ordinary cells. Thus in Ehrenberg's *Monadina* are to be found, I think, the following grades, perfectly analogous to the grades of cells:—

"1. Globules and discs.

"2. Discs with a pellucid point.

"3. The point dividing.

"4. Nucleated cells.

"5. The nuclei dividing and thus giving origin to

"6. Young cells, which are seen both within and escaped from parent cells.

"There really seems to have been much truth in the remark long since made by Oken, that animals are groups of bodies comparable to the Infusoria. The cell is itself a little organism; and cells coalesce to form a larger one.

"The remarks just made respecting fissiparous generation, I apprehend, may be applied to gemmiparous reproduction, or propagation by means of buds."

No doubt the minute Infusoria, which seem to have their development arrested at the first or nearest stage from the primitive cell-formation, offer close and striking analogies to the primitive cells out of which the higher animals and all their tissues are developed; but the very step which the Infusoria take beyond the primitive cell-stage, invests them with a specific character, as independent and distinct in its nature as that of the highest and most complicated organisms. No mere organic cell, destined for ulterior changes in a living organization, has a mouth armed with teeth, or provided with long tentacula. I will not lay stress on the alimentary canal and appended stomachs, which many still regard as "sub judice;" but the endowment of distinct organs of generation, for propagating their kind by fertile ova, raises the Polygastric Infusoria much above the mere organic cell.

In many of the larger species of Polygastria, radiated vesicles, subtransparent and colourless, generally two in number, and situated near the two extremities of the body, of a highly irritable nature, rapidly contracting and dilating, have been observed. *Roesel* first figured this contractile vesicle in the *Vorticella*. In *Euodon*, in addition to these vesicles, Ehrenberg likewise discerned another organ, of an oval shape, of a dull white colour, and of considerable size, placed in the middle of the abdomen. It is easily detected by the want of colour, when the animal has been well fed and its stomach filled. This organ is regarded as the testicle, and the contractile radiated bladders as the *Vesiculæ Seminales*. The ovarium occupies a more important share of the general cavity of the body. It fills all the interspaces of the stomachs and intestine which are not occupied by the male organs, and consists of a number of minute corpuscles, or nucleated cells, connected together in a reticulate form, generally of a green or pink, or some other bright colour, in well-fed healthy *Polygastria*.

The act of generation is attended with the destruction of the parent. The ripe ova burst through some part of the abdominal integument, and escape in a reticulated mass, together with the fertilizing fluid.

By virtue of these diversified modes of multiplication, the powers of propagation of these diminutive organized creatures may be truly said to be *immense*. Malthusian principles, or what are vulgarly so called, have no place in the economy of this department of organized nature. To the first great law imposed on created beings, "increase and multiply," none pay more active obedience than the Infusorial animalcules.

Attempts have been made to calculate approximately this rate of increase.

On the 14th of November, Ehrenberg divided a *Paramœcium aurelia*, a Polygastric animalcule measuring one-twelfth of a line in length, into four parts, which he placed in four separate glasses.

On the 17th of November, the glasses numbered 1 and 4 each contained an isolated paramœcium, swimming actively about. The pieces in numbers 3 and 4 had disappeared.

On the 18th there was no change.

On the 19th each animalcule presented a constriction across the middle of the body.

On the 20th No. 1. had propagated five individuals by transverse spontaneous division: in No. 4. eight individuals had in like manner been generated.

On the 21st no change had taken place.

On the 22d there were six nearly equal-sized individuals in No. 1., and eighteen individuals in No. 4.

On the 23d, the individuals were too numerous to be counted.

Thus it was demonstrated that this species of Polygastrian would continue for six days without any diminution of reproductive force, and that on one day a single individual twice divided, and one of its divisions effected a third fission.

A similar experiment on a *Stylonychia Mytilus*, an animalcule one-tenth of a line in length, was attended with nearly the same results; it was supplied with the green nutrient matter, consisting of the *Monas pulvisculus*, and on the fifth day the individuals generated by successive divisions were too numerous to be counted.

And now you may be disposed to ask: To what end is this discourse on the anatomy of beings too minute for ordinary vision, and of whose very existence we should be ignorant unless it were revealed to us by a powerful microscope? What part in nature can such apparently insignificant animalcules play, that can in any way interest us in their organisation, or repay us for the pains of acquiring a knowledge of it? I shall endeavour briefly to answer these questions. The Polygastric Infusoria, notwithstanding their extreme minuteness, take a great share in important offices of the economy of nature, on which our own well-being more or less immediately depends.

Consider their incredible numbers, their universal distribution, their insatiable voracity; and that it is the particles of decaying vegetable and animal bodies which they are appointed to devour and assimilate.

Surely we must in some degree be indebted to those ever active invisible scavengers for the salubrity of our atmosphere. Nor is this all: they perform a still more important office, in preventing the gradual diminution of the present amount of organised matter upon the earth. For when this matter is dissolved or suspended in water, in that state of comminution and decay which immediately precedes its final decomposition into the elementary gases, and its consequent return from the organic to the inorganic world, these wakeful members of nature's invisible police are everywhere ready to arrest the fugitive organised particles, and turn them back into the ascending stream of animal life. Having converted the dead and decomposing particles into their own living tissues, they themselves become the food of larger Infusoria, as the *Rotifera*, and of numerous other small animals, which in their turn are devoured by larger animals, as fishes; and thus a pabulum, fit for the nourishment of the highest organised beings, is brought back by a short route, from the extremity of the realms of organic matter.

There is no elementary and self-subsistent organic matter, as Buffon

taught: the inorganic elements into which the particles of organic matter pass by their final decomposition are organically recomposed, and fitted for the sustenance of animals, through the operations of the vegetable kingdom. No animal can subsist on inorganic matter. The vegetable kingdom thus stands, as it were, between animal matter and its ultimate destruction; but in this great office plants must derive most important assistance from the Polygastric *Infusoria*. These invisible animalcules may be compared, in the great organic world, to the minute capillaries in the microcosm of the animal body, receiving organic matter in its state of minutest subdivision, and when in full career to escape from the organic system, and turning it back by a new route towards the central and highest point of that system.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Continued Daylight within the Arctic Circle*—Nothing made so deep an impression upon our senses as the change from alternate day and night, to which we had been habituated from our infancy, to the continued daylight to which we were subjected as soon as we crossed the Arctic circle. Where the ground is but little trodden, even trifles are interesting; and I do not, therefore, hesitate to describe the feelings with which we regarded this change. The novelty, it must be admitted, was very agreeable; and the advantage of constant daylight, in an unexplored and naturally boisterous sea, was too great to allow us even to wish for a return of the alternations above alluded to; but the reluctance we felt to quit the deck when the sun was shining bright upon our sails, and to retire to our cabins to sleep, often deprived us of many hours of necessary rest; and when we returned to the deck to keep our night-watch, if it may be so called, and still found the sun gilding the sky, it seemed as if the day would never finish. What, therefore, at first promised to be so gratifying, soon threatened to become extremely irksome; and would, indeed, have been a serious inconvenience had we not followed the example of the feathery tribe, which we daily observed winging their way to roost, with a clock-work regularity, and retired to our cabin at the proper hour, where, shutting out the rays of the sun, we obtained that repose which the exercise of our duties required. At first sight it will, no doubt, appear to many persons that constant daylight must be a valuable acquisition in every country; but a little reflection

will, I think, be sufficient to shew that the reverse is really the case, and to satisfy a thinking mind that we cannot overrate the blessing we derive from the wholesome alternation of labour and rest, which is in a manner forced upon us by the succession of day and night. It is impossible, by removing to a high latitude, to witness the difficulty there is in the regulation of time; the proneness that is felt by the indefatigable and zealous to rivet themselves to their occupations, and by the indolent and procrastinating to postpone their duties, without being truly thankful for that allwise and merciful provision with which nature has endowed the more habitable portions of the globe.—*Voyage of Discovery towards the North Pole, in* 1818, *by Captain Beechey.*

2. *An attempt to explain the Phenomena of the Freezing Cavern at Orenburg.* By Dr Hope.*—This cavern is one of several caves which exist in the southern face of a lengthened low hillock of gypsum. It is entered from the south by a passage rather narrow, and is about fifteen feet high, ten paces long, and seven wide; which seemed to send off irregular fissures into the body of the rock.

The extraordinary feature of this cavern is, that during summer it is so cold that ice is generated in it, and dry icicles hang from its roof; and that, in winter, all appearance of congelation ceases, and the temperature becomes such that the Russians say they could sleep in it without their sheep-skins.

Mr Murchison applied to Sir John Herschel for an explanation, and the theory which he proposed is, that the heat and cold of the surface gradually move, though very slowly, backward into the rock; that it requires six months for the wave of cold, as he terms it, to reach the cavern, and consequently, that that frigid wave begins to arrive at the commencement of summer, and continues during that season, occasioning such a degree of cold in the cavern as to produce the congelations described by Mr Murchison.

At the commencement of winter, the first effect of the summer's heat arrives, and continues without interruption, and occasions warmth enough to prevent congelation.

Dr Hope entirely concurred with Sir John Herschel in thinking that alternate waves of heat and cold must exist and have a share in producing the phenomena, and in corroboration quoted the observations of Saussure, that at Geneva the winter's cold requires six months to descend 29½ feet, and that the summer's heat penetrates to the same depth in a similar period of time; the maximum of cold taking place at mid-summer, and of heat at mid-winter.

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* *Vide* vol. xxxiv. p. 10, Edinburgh New Philosophical Journal, for account of this cavern.

But he also expressed his conviction that these alternate waves were not sufficient to account for the phenomena, further remarking, that were they the only powers employed, the paradoxical phenomena should occur equally in some of the other caverns of the Orenburg hillock, or in other caverns in different quarters of the globe. He observed, that there must be something peculiar to the Illetykaya Zatchita cavern which renders it the only cave in the world which possesses the singular property, so far as he knew. He then alluded to the caverns in different parts of the globe in which accumulations of snow are found in summer, and concurred with Mr Murchison in thinking that they have no analogy with that of Orenburg. They are merely receptacles of the winter snow and ice, and preserve it during summer, after the manner of an ice-house.

The circumstance peculiar to the Orenburg cave is the occurrence of the rents and fissures which rise from the back part of the cavern.

The author stated, that if it were granted that these fissures reach the surface, even by the smallest ramifications, and that they ascend within the reach of the alternate waves of heat and cold, the whole phenomena may be easily and satisfactorily explained. He ascribed the summer's coldness and congelation to a constant current of cold air through the fissures of the rock into the cavern; and he supposed that the current is occasioned in the following manner: When at the close of spring the temperature of the external air and of that in the rents is the same, no particular occurrence takes place, but as soon as the wave of cold begins to make impression on the rocky parietes of the fissures, then the air in them will be somewhat cooled, contracted, and rendered specifically heavier. This being so, the weight of the column of air in these rents will be greater than that of a column of equal altitude of the external atmospheric air, and the consequence will necessarily be, that the colder air will descend, the warmer atmospheric air from above will supply its place, which, in its turn, will be cooled and descend, and thus a current of cold air through the crevices into and through the cavern will be established. As the temperature of the rocky parietes gradually falls with each successive wave of cold, the air in the fissures will become colder and colder, and in the same proportion will descend more rapidly.

But the rapidity of descent does not only depend upon the increasing coldness of the air in the fissures, but is further augmented by the warmth of the summer expanding the external air, so that the difference of weight between the external and internal columns becomes greater. In the manner now explained, a current of cold air is constantly descending and flowing through the cavern, producing all the surprising frigorific effects displayed within it.

That such a current does exist, Mr Murchison gives a satisfactory proof. He says, "That, upon unlocking the frail door of the cavern, a volume of air, so surpassingly keen, struck the legs and the feet, that he was glad to rush into a cold bath in front of him to equalize the effect." This downward current will continue the same till the close of autumn, when its course comes to be changed, by that time the first approaches of May's surface warmth will begin to be experienced, the cold of the sides of the rents begins to diminish, and the temperature of the external air must have fallen to nearly that of the internal current. As soon as an equality between the temperatures and densities of the external and internal columns shall have been established, all current must cease. At this period, namely, the commencement of winter, the wave of the summer s heat begins to reach both the walls of the air channels and of the cavern, and gradually communicates a warmth which progressively elevates the temperature, and dissipates every mark of the preceding summer's congelation. It might at first be reasonably expected, that at this time the preceding order of things would be reversed, and that a current in the opposite direction would commence, such as, it is known, happens in many mines; for, undoubtedly, the temperature of the atmosphere descending rapidly, the gravity of the external air would soon exceed that of the internal column. A current would immediately commence from below, and, entering from the cavern door and ascending through the rents, escape at the surface. The consequence of such a current would be, that the cold would soon reappear in the cave, and gradually increase during the severity of the winter, and completely overpower the heating influence of the thermal wave, now beginning to operate on the walls of the cavern, and so prevent the warmth of the cave during winter.

An occurrence, however, now takes place which puts a stop to the upward draught, and permits the thermal wave to have its full influence on the temperature of the cavern. The winter commences with repeated falls of snow, which form a thick covering on the surface of the earth, and closes up all the communications between the extremities of the crevices and the external air, and no current can take place. In this manner the influx of the intensely cold air into the cavern, and its ascent through the fissures, is prevented, and then full play is given to the calorific power of the wave of heat which continues to arrive in the cavern through its rocky sides during the whole continuance of winter, and communicates the warmth recorded by Mr Murchison. In the beginning of summer the snows melt, and the terminations and ramifications of the fissures have their communication with the atmosphere restored. The currents, as already described, are re-established, and all the paradoxical phenomena to which they give birth present themselves in due succession.

3. *Division of Seasons, of Demerara and Essequibo, British Guiana.—The Seasons*, which *only* consist of *two*, are divided into

The greater and lesser wet,—and
The greater and lesser dry.

The greater or longer dry season commences in *July*, and continues until *November;* the range of temperature, as shewn at this period by the thermometer, is from 80° to 90°, sometimes 91° (but very seldom) in the shade.

From the severe and long-continued drought existing at this season, and consequent want of moisture in the atmosphere, the earth becomes sterile and unproductive, and the progress of vegetation becomes as much suspended as it does during the winter in the more northerly latitudes.

It is a singular circumstance, but not less singular than true, although not noticed by any author, that during the continuance of the *long dry season*, night-dews are of rare occurrence.

The short dry season extends from February to April, or Easter.

The greater wet season then begins and continues, generally speaking, during April, May, June, and not unfrequently part of July.

The lesser wet season commences in December, and continues until February.

According to Count Robert H. Schomburgk's estimate, "the mean temperature for the year is 81° 2′; the maximum 90°; the minimum 74.°"

Georgetown is situated in lat. 6° 49′ 20″ N.; and long. 58° 11′ 30″ N.

P. S.—The coldest and most unhealthy wind is that which blows from the north-west. I invariably suffered from headache, and other unpleasant symptoms of indisposition, whenever the wind shifted or blew from that quarter.

William Fraser, M.D.
Late of Demerara.

4. *Magdalena Bay in Spitzbergen.*[*] Magdalena Bay was the first port in which we had anchored in the Polar regions, and there were, of course, many objects to engage our attention. We were particularly struck with the brilliancy of the atmosphere, the peaceful novelty of the scene, and the grandeur of the various objects with which nature has stored these unfrequented regions.

The anchorage is bounded by rugged mountains, which rise precipitously to the height of about 3000 feet. Deep valleys and glens occur between the ranges, the greater part of which are either filled with immense beds of snow, or with glaciers, sloping from the summits of the mountainous margin to the very edge of the sea. Owing to the westerly direction of these ranges, and the precipitancy with which they rise, the sun never shines upon the southern shore of the bay, with the exception of a few hours about midnight during the height of summer, and then only at a very low altitude ; whereas its rays are exerted with the fullest effect upon the northern shore, which occasionally radiates a heat of 57 or 60 degrees. There is, consequently, the most marked difference between the sides of the bay, both in point of climate and general appearance ; for while, on the one, perpetual frost is converting into ice the streams of water occasioned by the thawing of snow upon the upper parts of the mountains which are exposed to the sun's rays, the other side is relieving itself of its superficial winter crust, and refreshing a vigorous vegetation with its moisture.

This process of contemporaneous thawing and freezing seems, as I shall immediately take occasion to explain, to have been very instrumental in the formation of those stupendous glaciers, which strike with astonishment and admiration every person who has an opportunity of beholding them.

In Magdalena Bay there are four of these glaciers, two of which are situated on the southern shore, at the margin of the sea. The third, which I have mentioned as bearing the appropriate name of " the Hanging Iceberg," appears to have accumulated without any lateral support, as though a stream of water had issued from a particular spot, and become congealed as it descended; thus forming a nucleus, which gradually increased, and rose as the stream poured its waters over its accumulating surface, until, in the course of ages, the mass has attained its present bulky dimensions.

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[*] From Captain Beechey's delightful volume, entitled " A Voyage of Discovery towards the North Pole," &c. London, 1843.

The fourth and largest occupies the head of the bay, and extends from two to three miles inland. Numerous large rents in its upper surface, occasioned, perhaps, either by its own motion, or by the subsidence of its foundation, have caused it to be gratuitously named the "Waggon Way," in accordance with the supposed resemblance which these fissures bear to the ruts left by the waggon.

From the circumstance of the sea being of great depth immediately off these glaciers, they are prevented making an undue encroachment upon the bay, and, indeed, from filling it up, which, if the water were shallow, would, in the course of time, inevitably be the case, either by the grounding of the pieces which break away from the frontage, or by the berg finding a foundation to advance upon. At present, the warmth of the sea prevents the accumulation of the ice below a certain depth, and, during the summer, so far undermines the accumulation of the winter, that large masses fall off by their own superincumbent weight, and are carried out to sea; so that the berg is thus kept within due bounds. The frontage of the *waggon way* presents a perpendicular surface of 300 feet high, by 7000 feet in length. Nevertheless, upon so gigantic a scale is all nature around, that, although of these stupendous dimensions, neither this glacier, nor any of the numerous and beautiful variety, creates much astonishment in the mind of the beholder, until he approaches within the influence of the *blink* or luminous haze, which is invariably radiated by large masses of ice. At this distance, the wall of ice has an awfully grand appearance, heightened perhaps by a sense of the personal danger to which so near an approach must expose the spectator; for large pieces have occasionally broken away from this berg, which have done considerable mischief. The soft blue tint of the surface of the ice is here also clearly discerned, whilst the long, sparkling icicles, pendant from the roofs of the caverns, and a variety of curious shapes, which may also be traced on the face of the glacier, serve greatly to increase the interest and admiration.

On a perfectly calm day, when the blink of the ice is strong, a curious deception is produced by the combined effect of the ice below the surface of the water, and the perfect reflection of that above. The sea presents a white, creamy appearance; the seals sporting on its surface, seem to be swimming in a thick milky substance; and the ripple, as it sweeps along, occasions long white lines, so that it is only by looking perpendicularly upon the water around the boat that its transparency is perceived, and the deception is detected.

In another part of my Journal, it is shewn that the danger of approaching these fragile masses of ice is far from imaginary, and that there is also a necessity for a strict observance of silence in their immediate vicinity. The fact is, that, as the berg is constantly breaking away during summer, there are generally some pieces all but on the point of falling, and capable of being detached by the smallest concussion of the air; the explosion of a gun scarcely ever failing to bring down one of the masses.

In cloudy or misty weather, when the hills are clothed with newly fallen snow, nothing can be more dreary than the appearance of the shores of Spitzbergen; whereas, on the contrary, it is scarcely possible to conceive a more brilliant and lively effect than that which occurs on a fine day, when the sun shines forth and blends its rays with that peculiarly soft, bright atmosphere which overhangs a country deeply-bedded in snow; and with a pure sky, whose azure hue is so intense as to find no parallel in nature. On such an occasion, the winds, near the land at least, are very light, or entirely hushed, and the shores teem with living objects. All nature seems to acknowledge the glorious sunshine, and the animated part of the creation to set no bounds to its delight. Such a day was the 4th of June, and we felt most sensibly the change from the gloomy atmosphere of the open sea, to the cheerful glow that overhung the hills and placid surface of Magdalena Bay.

Although surrounded by beds of snow and glaciers, with the thermometer scarcely above the freezing point, there was no sensation of cold. The various amphibious animals, and myriads of birds which had resorted to the place, seemed to enjoy, in the highest degree, the transition thus occasioned by a few bright hours of sunshine. From an early hour in the morning, until the period of rest returned, the shores around us reverberated with the merry cry of the little auk, willocks, divers, cormorants, gulls, and other aquatic birds; and wherever we went, groups of walruses, basking in the sun, mingled their playful roar with the husky bark of the seal.

There was certainly no harmony in this strange din; but it was at the least gratifying to know that it arose from a demonstration of happy feelings. It was a pleasure of the same character as that which must have been experienced by every traveller, who, on some fine bright evening, in a tropical climate, has listened to the merry buzz of thousands of winged insects which immediately succeeds the setting of the sun. And here we cannot fail to notice the manner in which the great Author of Nature has varied His dispensations. In the burning region of the torrid zone, the descent of the sun calls into action myriads of little beings which could not exist under the fierce glare of his meridian ray; whereas here, on the contrary, it is the signal for universal repose.

This period of the day had no sooner arrived in Magdalena Bay, than there was a stillness which bordered on the sublime,—a stillness which was interrupted only by the bursting of an iceberg, or the report of some fragment of rock loosened from its hold. These sounds, indeed, which came looming over the placid surface of the bay, could hardly be considered interruptions to the general silence; for, speedily dying away in the distance, they left behind a stillness even more profound than before.

In the day-time the presence of our expedition was not disregarded. The birds shunned us in their flight, and every noise which was occasionally made, sounding strange to the place, sent to a greater distance the sea-gulls that were fishing among the rocks, and kept on the alert the whole herds of animals, many of which would otherwise have been lost in sleep; causing them to raise their heads when anything fell upon our deck, and to cast a searching look all over the bay, as if to inquire whence so unusual a disturbance proceeded. These little alarms, which would have passed unheeded in situations frequented by man, proved, more than any other incident, how great a stranger he was in these regions; a feeling which, I must confess, carried with it an agreeable sensation, arising, no doubt, from the conviction that we were treading a ground which had been but rarely visited before.

When we first rowed into this bay, it was in quiet possession of herds of walruses, who were so unaccustomed to the sight of a boat, that they assembled about her, apparently highly incensed at the intrusion, and swam towards her as though they would have torn the planks asunder with their tusks. The wounds that were inflicted only served to increase their rage; and I frankly admit, that, when I considered how many miles we were from our vessel, and what might be the result of this onset, I wished we had the support of a second boat. We continued, however, to keep them off with our fire-arms, and fortunately came off without any accident. When we afterwards came to anchor, we went better provided, and succeeded in killing several of these animals upon the ice at the head of the bay.

We found some of these monsters fourteen feet in length and nine feet girth, and of such prodigious weight that we could scarcely turn them over. In the inside of several there were round granite pebbles, larger than walnuts, and in one we counted two-and-twenty. Their hides were so tough, that a bayonet was the only weapon which would pierce them; and we were not a little surprised at the accounts of the early voyagers already mentioned, in which it is stated, that a thousand of these animals were killed in the short space of seven hours, by the crew of one vessel. Nor were we less curious to find out the manner in which they had contrived to pen up on the shore five hundred walruses alive, and keep them prisoners for several days, as appears to have been the case in one of the voyages alluded to. I can only say, that, had such a task been imposed upon us, we should have found it utterly impossible to accomplish it.

NEW PUBLICATIONS RECEIVED.

The following Publications have appeared, or are about to appear:—

1. *Professor Forbes' Work on the Alps.*—We announce with much pleasure the early publication of Professor Forbes' Travels through the Alps of Savoy, and other parts of the Pennine Chain, with Observations on the Phenomena of Glaciers; a work which, we feel convinced, will be alike interesting to the man of science, the general reader, and the traveller in the Alps. Besides the valuable chapters giving the results of the author's laborious and ably conducted investigations on glaciers, the volume contains much that is new and curious regarding the Physical Geography, Geology, and Meteorology of the Alps, and includes narratives of expeditions to many of the least frequented and most picturesque scenes, and to some of the most difficult Passes in the Pennine Chain, such as the Glaciers of Miage and La Brenva, the Passage of the Col du Géant, the Val Pelline, the Col de Fenetres, the Col de Côllon, the Glacier of Ferpecle, the environs of Zermatt, the Col of Mont Cervin, Macugnaga, Monte Moro, &c. The volume is illustrated by beautiful lithographed views, wood-engravings, &c., and, above all, by a large and finely executed Map of the *Mer de Glace* of Chamouni, constructed from the

materials obtained by Professor Forbes during his minute survey of that important glacier.

2. Sur les vertebres de Squales vivans et fossiles par J. Muller and L. Agassiz, Neuchatel, 1843. 4to.

3. The Naturalist's Library, conducted by Sir William Jardine, Bart., F.R.S.E., F.L.S., F.W.S., &c. Two volumes only remain to be published of this beautifully illustrated and cheap work. We understand it has been eminently successful, and we therefore expect the authors and publishers will select some new subjects, and gratify the public by a New Series.

4. Nomenclator Zoologicus, continens Nomena Systematica Generum Animalium tam Viventium quam Fossilium, secundum Ordinem alphabeticum disposita. Auctore L. Agassiz, Fasciculus III. & IV. Continens Crustacea et Vermes I. E. Entozoa, Turbellaria et Annulata, Hemiptera et Infusoria; Polygastrica et Rotatoria. Quarto, 1843.

5. The Zoology of the Voyage of H M. S. Sulphur, under the command of Captain Sir Edward Belcher, R. N., &c. during the years 1836–42. Published under the authority of the Lords Commissioners of the Admiralty. Edited and superintended by Richard Brinsley Hinds, Esq. surgeon, R. N., attached to the Expedition. Mammalia. By John Edward Gray, Esq., F.R.S., &c. Quarto. Of this very beautiful and interesting work, No. I. of The Mammalia is now before us. London: Smith, Elder, and Co., 65 Cornhill. 1843.

6. Description of an Extinct Lacertine Reptile, Rynchosaurus articeps (Owen), of which the bones and foot-prints characterize the upper new red sandstone at Grinsell, near Shrewsbury. By Professor Owen. Quarto. 1842. Parker, Cambridge.

7. Recherches sur la Croissance du Pin Sylvestre dans Le Nord de l'Europe. Par A. Bravais et Ch. Martins, Membres de la Commission du Nord. 2to. Bruxelles.

8. Scripture Geology. Part 2d. By Rev. William White. Edinburgh, Bell and Bradfute. 1834.

9. Description of Whitelaw and Stirrat's patent Water-Mill, with an account of the performances of a number of these machines. London, Mechanics' Magazine Office. 8vo, pp. 64, with plates.

10. Fourth Annual Report of the Registrar-General, of Births, Deaths, and Marriages in England. London, printed by W. Clowes and Sons, for Her Majesty's Stationary Office. 1842. 8vo, pp. 361.

11. Report to Her Majesty's Principal Secretary of State for the Home Department, from the Poor Law Commissioners, on an In-

quiry into the Sanitary Condition of the Labouring Population of Great Britain; with Appendices. Presented to both Houses of Parliament by Command of Her Majesty, July 1842. 8vo, pp. 457. Printed by Clowes and Sons, for Her Majesty's Stationary Office.

12. Address delivered at the Anniversary Meeting of the Geological Society, London, on the 17th February 1843. By Roderick Impey Murchison, F.R.S., President of the Society. Royal 8vo, pp. 119. London, Richard and John Taylor. 1843.

13. An Inaugural Lecture on Botany, considered as a Science and as a branch of Medical Education. Read in King's College, London, May 8. 1843, by Edward Forbes, F.L.S., F.B.S., Vice-President of the Wernerian Natural History Society, &c.; Professor of Botany in King's College, London. London, J. Van Voorst, 1 Paternoster Row.

14. An Address delivered to the Berwickshire Naturalists' Club, at its Anniversary Meeting held at Lowick, September 28. 1842, by George Darling, Esq., President.

15. Ancient Irish Pavement Tiles, exhibiting Thirty-Two Patterns, illustrated by Forty Engravings, after the originals existing in St Patrick's Cathedral, and Howth, Mellifont, and Newton Abbeys. By Thomas Oldham, A.B., F.G.S. L. and D. Quarto. Dublin, John Robertson, Grafton Street; Longman & Co. London; J. H. Parker, Oxford; and J. Johnstone, Edinburgh.

16. The Revenue in Jeopardy from Spurious Chemistry, demonstrated in Researches upon Wood-Spirit and Vinous-Spirit. By Andrew Ure, M.D., F.R.S., &c., Analytical Chemist to the Board of Customs. London, Ridgway. 1843.

17. Manual of British Botany, containing the Flowering Plants and Ferns arranged according to the Natural Orders. By Charles C. Babington, M.A., F.L.S., F.G.S., &c. 12mo, pp. 400. London, John Van Voorst. 1843.

18. Thoughts on the Mental Functions; being an Attempt to treat Metaphysics as a branch of the Physiology of the Nervous System. 12mo, pp. 254. Oliver and Boyd, Edinburgh, and Simpkin, Marshall, and Co., London. 1843.

19. Popular Cyclopædia of Natural Science. By William B. Carpenter, M.D. London, W. S. Orr and Co. 1843. *This judiciously conducted work will, we trust, ere long find its way into our libraries and public seminaries.*

20. Examination of the Cowdie Pine Resin. By Robert Dundas Thomson, M.D. 1843.

List of Patents for Inventions granted for Scotland from 23d March to 23d June 1843 *inclusive.*

1. To Gregory Seale Walters, of Coleman Street, in the City of London, merchant, being a communication from abroad, "improvements in the manufacture of chlorine and chlorides, and in obtaining the oxides and peroxides of manganese in the residuary liquids of such manufacture."—23d March 1843.

2. To James Greenshields, of Monteith Row, Glasgow, gentleman, "improvements in the manufacture of compositions for covering roads, streets, and other ways and surfaces, and in rendering fabrics waterproof, to be used for covering buildings, bales, packages, and for other useful purposes."—23d March 1843.

3. To Andrew Barclay, engineer and brassfounder, Kilmarnock, in the county of Ayr, Scotland, "certain improvements in lustres, chandeliers, pendants, and apparatus connected therewith, to be used with gas, oil, and other substances, which invention is also applicable to other purposes."—24th March 1843.

4. To James Fletcher, foreman at the works of Messrs W. Collier and Co., engineers, of Salford, in the county of Lancaster, "certain improvements in machinery, or apparatus for spinning cotton and other fibrous substances."—27th March 1843.

5. To William Henry James of Martin's Lane, in the city of London, civil-engineer, "certain improvements in railways, and carriage-ways, railways, and other carriages, and in the modes of propelling the said carriages, parts of which improvements are applicable to the reduction of friction in other machines."—27th March 1843.

6. To Claud Edward Deustche, of Fricour's Hotel, St Martin's Lane, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in combining materials to be used for cementing purposes, and for the preventing the passage of fluids, and also for forming and constructing articles from such compositions of materials."—30th March 1843.

7. To John Juckes, of Putney, in the county of Surrey, gentleman, "improvements in furnaces."—30th March 1843.

8. To Thomas Edge, of Great Peter Street, in the city of Westminster, gas-apparatus manufacturer, "certain improvements in apparatus for measuring gas, water, and other fluids."—30th March 1843.

9. To Robert William Sievier, of Henrietta Street, Cavendish Square, in the county of Middlesex, gentlemen, "certain improvements in looms for weaving, and in the mode or method of producing plain or figured goods or fabrics."—3d April 1843.

10. To James Byrom, of Liverpool, in the county of Lancaster, engineer, "an improved system of connexion for working the cranks of what are commonly called direct-action steam-engines."—3d April 1843.

11. To Peter Kagenbusch, of Wetter-on-Rhur, in Westphalia, in the kingdom of Prussia, dyer, now residing in the parish of Lyth, in the county of York in England, "certain improvements in the treatment of the alum rock or schist, and in the manufacture and application of the products derived therefrom."—6th April 1843.

12. To Robert Faraday of Wardour Street, Soho, in the county of

Middlesex, gas-fitter, being a communication from abroad, "improvements in ventilating gas-burners, and burners for consuming oil, tallow, or other matters."—6th April 1843.

13. To William Barnard Boddy, of the parish of St Mary, Newington, in the county of Surrey, surgeon, "improvements in apparatus, and means, for opening, shutting, and fastening every description of sliding and lifting window-sashes, windows, and window shutters."—11th April 1843.

14. To Charles Frederick Guitard, of Birchen Lane, in the city of London, notary-public, being a communication from abroad, "certain improvements in the construction of railways, and of rail way carriages.' —19th April 1843.

15. To William Edward Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil-engineer, being a communication from abroad, "certain improvements in the construction of boxes for the axles or axle-trees of locomotive engines and carriages, and for the bearings or journals of machinery in general, and also improvements in oiling or lubricating the same."—26th April 1843.

16. To Nicolas Henri Jean Francois, Comte de Crony, of Connaught Terrace, in the county of Middlesex, "certain improvements in rotary pumps and rotary steam-engines."—28th April 1843.

17. To Hinrik Zander, of North Street, in the county of Middlesex, gentleman, "certain improvements in steam-engines, boilers and furnaces, and in the method of feeding the same, as also in the machinery for applying steam power to propelling purposes."—2d May 1843.

18. To Pierre Pelletan, of Bedford Square, in the county of Middlesex, Esquire, "improvements in the production of light."—4th May 1843.

19. To William Mayo, of Lower Clapton, and John Warmington, of Wandsworth Road, gentleman, being a communication from abroad, "improvements in the means of, and apparatus for, manufacturing gaseous liquors, and for filling bottles and other vessels used for holding the same, and retaining the contents therein, and emptying the same when required."—4th May 1843.

20. To Isham Baggs, of Wharton Street, in the county of Middlesex, chemist, "improvements in the production of light."—9th May 1843.

21. To Andre Eustache Gratien Auguste Maurras, of Cornhill, in the city of London, gentleman, "certain improvements in the process and apparatus for filtering water and other liquids, a part of which improvements are his invention, and the remainder communicated to him by a foreigner residing abroad."—17th May 1843.

22. To Charles Maurice Elizei Sautter, of Austin Friars, in the city of London, gentleman, being a communication from abroad, "improvements in the manufacture of borax."—23d May 1843.

23. To John Laing, of Dundee, in the county of Forfar, linen manufacturer, "improvements in apparatus for rubbing linen-cloth, when making in power-looms."—23d May 1843.

24. To John Nisbett, of Elm Street, Long Lane, Bermondsey, in the county of Surrey, engineer, "improvements in preparing hides and skins in the manufacture of certain descriptions of leather."—23d May 1843.

25. To Joseph Burch, of City Road, in the county of Middlesex, civil engineer and machinist, "certain improvements in machinery for printing on cotton, silk, woollen, paper, oil-cloth, and other fabrics, and materials, and certain apparatus to be used in preparing the moulds, and casting

surfaces for printing, and for certain modes of preparing surfaces previously to the design being delineated upon them."—23d May 1843.

26. To Angier March Perkins, of Great Coram Street, in the county of Middlesex, engineer, "improvements in the manufacture and melting of iron, which improvements are applicable for evaporating of fluids and disinfecting oils."—25th May 1843.

27. To William Brown, of the city of Glasgow, "improvements in the manufacture of porcelain, china, pottery, and earthenware, and which improvements are also in part applicable to the manufacture of paper, and to the preparation of certain pigments or painter colours."—26th May 1843.

28. To Percival Moses Parsons, of Stamford Street, in the county of Surrey, civil engineer, "certain improvements in steam-engines and boilers, and in motive machinery connected therewith."—31st May 1843.

29. To Alfred Brewer, of Surrey Place, Old Kent Road, in the county of Surrey, wire-worker and felt manufacturer, "improvements in machinery for manufacturing paper, being a communication from abroad."—1st June 1843.

30. To Charles Clark, of No. 1. Great Winchester Street, in the city of London, merchant, "an improved pyro-hydro pneumatic apparatus, or means of generating, purifying, and condensing steam and other vapours, and of extracting from vegetable substances the soluble portions thereof; as also the application of parts of the said apparatus to other heating, evaporating, and distilling purposes."—3d June 1843.

31. To John Tappan, of Fitzroy Square, in the county of Middlesex, gentleman, being a communication from abroad, "certain improvements in machinery for preparing and spinning hemp and such other fibrous materials as the same is applicable to."—5th June 1843.

32. To Joseph Beaman, of Smethwick, in the county of Stafford, ironmaster, "improvements in the manufacture of malleable iron."—7th June 1843.

32. To James Boydell junior, of Old Farm Iron Works, near Dudley, ironmaster, "improvements in manufacturing bars of iron with other metals."—7th June 1843.

34. To Robert Alexander Kennedy, of Manchester, in the county of Lancaster, cotton-spinner, "certain improvements in machinery for grinding or sharpening cards used in carding cotton or other fibrous materials."—7th June 1843.

35. To Martyn John Roberts, Esq. of Bryn-y-caeran, in the county of Carmarthen, Esq. "certain improvements in machinery for preparing, spinning, and winding wool, cotton, flax, silk, or any other fibrous bodies."—8th June 1843.

36. To Charles Hancock, of Grosvenor Place, in the county of Middlesex, artist, "certain improvements in printing cotton, silk, woollen, and other fabrics."—13th June 1842.

37. To George Robins Booth, of Hanley, in the county of Stafford, manufacturer and chemist, "a certain improved mode of applying heat from various combustibles to manufacturing and other useful purposes."—15th June 1843.

Edin. New Phil. Journal. Plate II. Vol XXXV. p. 51.

MAXTON'S SELF-ACTING STOPPER *for* WINDING-ENGINES

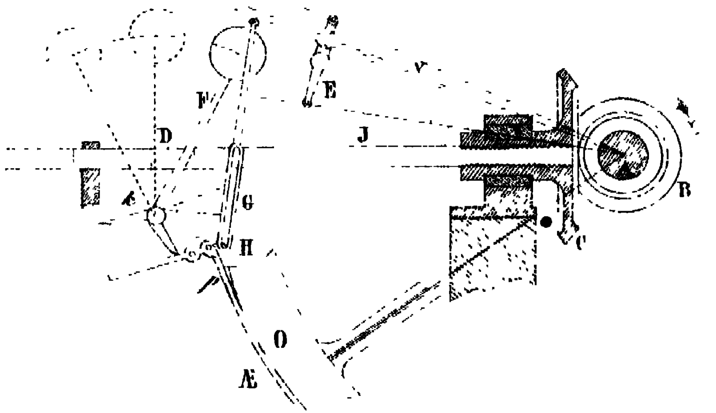

Fig. 1.

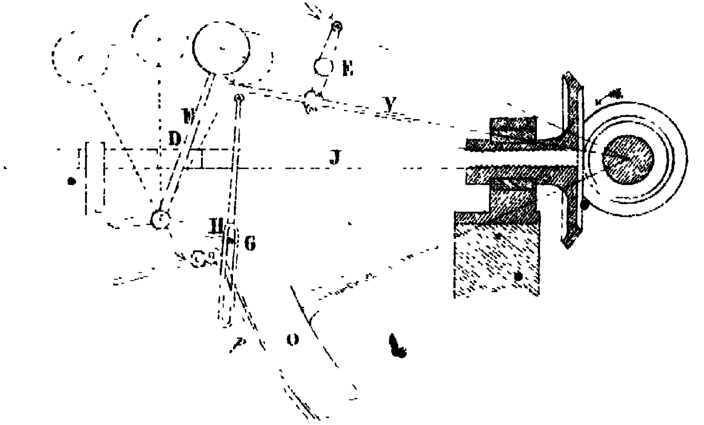

Fig. 2.

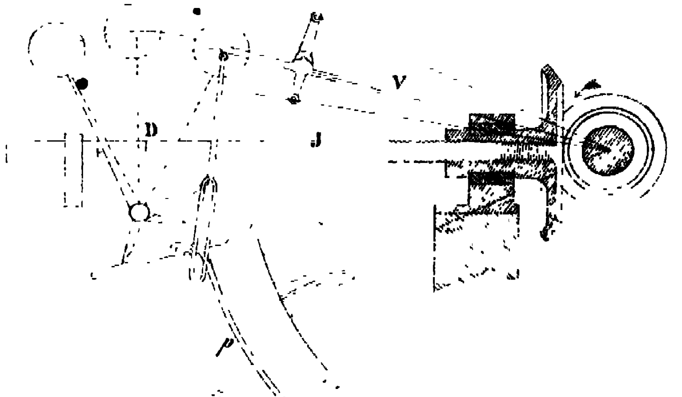

Fig. 3.

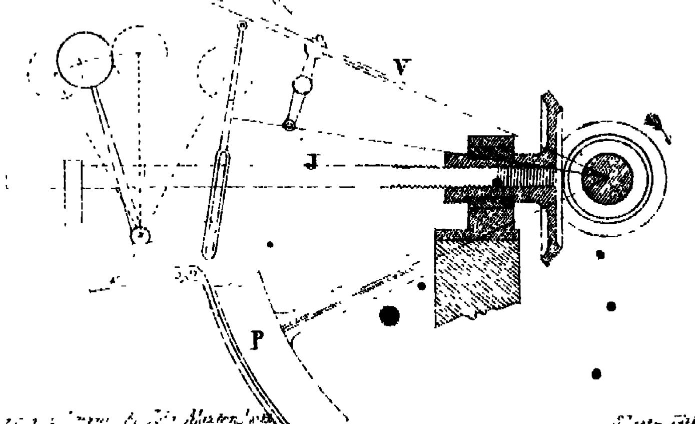

Fig. 4.

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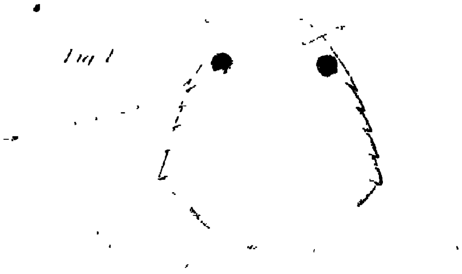
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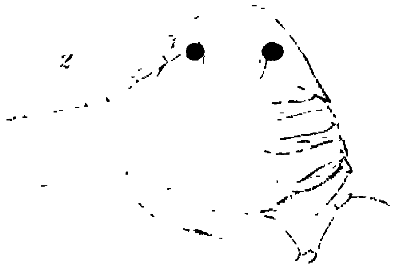
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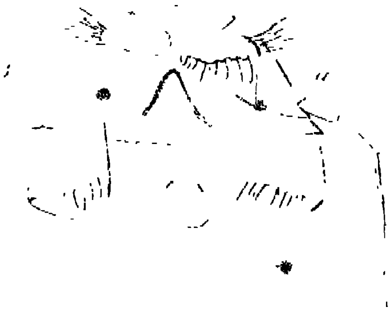
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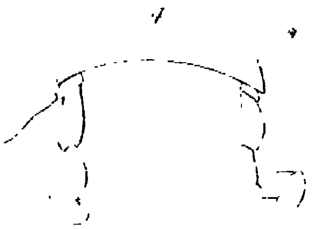
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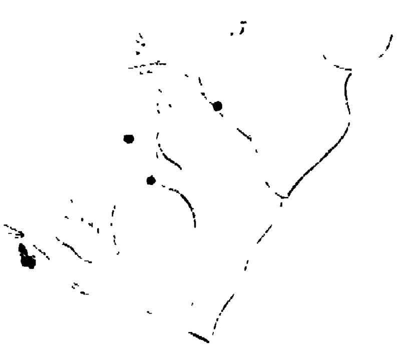
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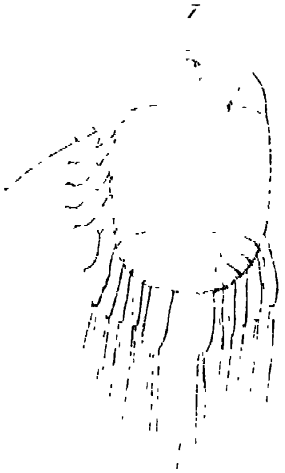
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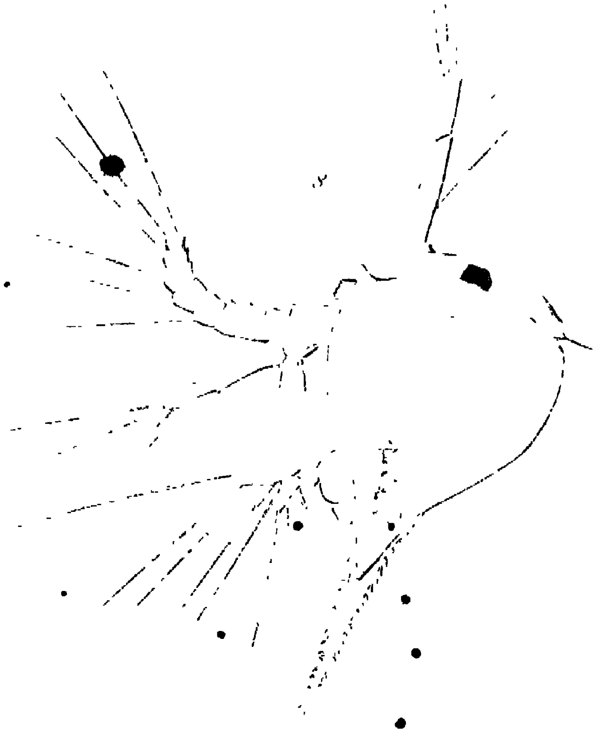
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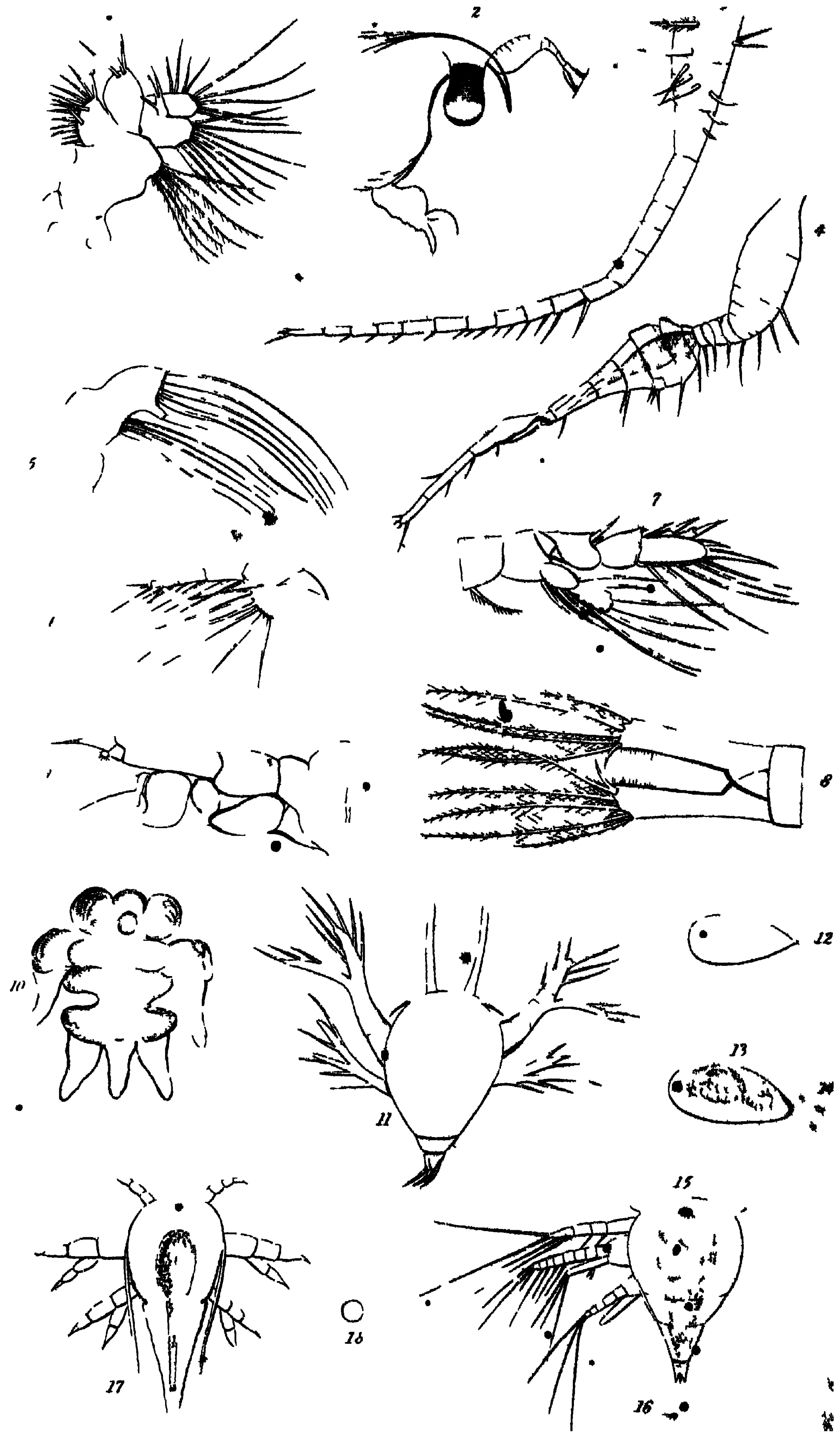

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PHILOSOPHICAL JOURNAL.

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*On Fissiparous Generation.** By MARTIN BARRY, M.D. F.R.SS. L and E., M.W.S., &c. With a Plate. (Communicated by the Author.)

THE first eight paragraphs of the following memoir, and the notes appended to them, were submitted to Dr Bostock and Professor Owen in April 1842, at which time I intended to present them to the Royal Society, as part of an addition to a paper that has since been printed in its Transactions. I afterwards withdrew them, as more properly belonging to the subject of the present communication. The eight paragraphs in question are these

1 Among the facts adduced to shew the resemblance between the blood-corpuscle and the germinal vesicle, I mentioned that, in certain states, an orifice is to be discerned in the centre of the parietal nucleus of both. The resemblance here is very remarkable indeed. With regard to the germinal vesicle, strong presumptive evidence was brought forward to shew, that a substance of some sort is introduced by the orifice in question; and from this circumstance, I denominated the centre of the nucleus the point of fecundation. Can it be, that the corpuscles of the blood undergo a sort of fecundation through the corresponding orifice? The blood-

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* From a paper read before the Royal Society of London, 16th Feb. 1843.

corpuscle, like the germinal vesicle, is also propagated by self-division of its nucleus.

2. The same mode of propagation—namely, by means of parent cells, self-division of the nucleus for this purpose, and an orifice in the centre of the nucleus—so far as my observations have extended, are common to cells in general. So that the Harveyan dictum—*Omne vivum ex ovo,* may be applicable to the very cells of which an organism is composed.

3. Professor Ehrenberg is of opinion, that reproduction by self-division necessarily produces in the offspring similarity to the progenitors. This remark has reference to some of the Infusoria. Now, if what Ehrenberg said of self-division, as occurring in the entire organism, be just, I think it may, to a certain extent, apply to the individual cells and nuclei of which the organism is composed; for, as I have already stated, cells are propagated by self division of their nuclei

4. The ovum is fecundated by the introduction of a substance into the centre of the nucleus of the germinal vesicle, or original parent cell, which then gives origin to two young cells. As it may be presumed that each of these young cells is endowed with qualities resulting from the fecundation of the parent cell, what I wish to be understood as suggesting is, that such endowment of the young cells may be referred to *self-division.** This division, however, as we saw, does not consist in simple separation, but is effected by a process elaborate in the extreme. The parent nucleus gives origin to many cells, and these to a great number of minuter cells, all of which disappear by entering into the formation of the two young persistent cells. The process seems to be one of *assimilation,* on which depends the re-appearance of the qualities of both parents in the offspring.

5. Now, it is deserving of notice, in the first place, that this same elaborate process of assimilation is seen to attend the reproduction of cells in general, so far as their interior can be

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* It is curious, that the original position of the young nuclei is sometimes seen to be such as to induce the belief that the *orifice* itself of the parent nucleus has undergone division (See Phil. Trans., 1842, Pl. X, fig 134)

discerned; and, secondly, that cells in general present a corresponding orifice in the centre of the nucleus, as if provided for the introduction of the substance to be so assimilated *

6. I conceive that what is seen taking place *in the manner here referred to*,—first in the germinal vesicle, and then in the individual cells, the descendants of this vesicle,—is not unconnected with what we observe in the reproduction of the entire organism—namely, a mysterious re-appearance of the qualities of both parents in the offspring, manifesting itself, as this re-appearance does, in the assemblage and metamorphoses of the cells.†

7 I have thus referred somewhat minutely to facts which I had previously mentioned, for the purpose of shewing that they may assist to explain a process described in the foregoing paper.‡ There, as well as in one of my former memoirs, certain nuclei are delineated as contained within and among the fibres of tissues § Corresponding nuclei have been seen, figured, and described by others; and it has been conjectured that they are the source of new substance, but the office which, more particularly, these nuclei perform, appears not to have been explained. I conceive them to be *centres of assimilation*, having been led to this opinion by observing, in the first place, that they present the remarkable orifice in question; and, secondly, that they are reproduced by self-division They descend in this manner from the nuclei of the original

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* That part of the nucleus to which the orifice leads, is the part where there is a continual origin of finely granular substance (See my "Researches in Embryology, Third Series" Phil Trans, 1840, p 549, par 385, and the description of figs 43 and 45, in my paper "On the Corpuscles of the Blood, Part II," Phil Trans 1841) The fact, too, that nuclei are found for a while at the surface of their cells, suggests the idea that they may remain there for a purpose analogous to that for which the germinal spot continues, up to a certain period, at the surface of its vesicle in the ovum.

† Cells, according to my observations, being propagated by division of their nuclei, it appears to me, that in reality, there is but one mode of reproduction namely, the fissiparous for what is called the highest organism, even after fecundation, is originally a simple cell

‡ See the introductory paragraph.

§ Phil. Trans. 1842 Pl vi. figs 22, 23, Pl. vii figs 35, 39, 42–44, 57, Pl viii figs. 68, Pl x figs 130, 132, 133–137, Pl xi figs. 155, Phil. Trans. 1841, Pl. xxiii figs 135–137, Pl. xxv figs. 157–159

cells of development;[*] *i. e.* from the nuclei of the corpuscles of the blood. That they are the source of new substance, is very obvious; for they may be seen either unwinding into a filament, or becoming spindle-shaped to form one.[†] But what I wish to add is, that the origin of new filaments in these nuclei appears to me to have especial reference to that assimilation of which they seem to be the centres.

8. Although every nucleus seems to possess a reproductive property, there are thus special centres of reproduction. Such centres were also particularly indicated in one of my former memoirs,[‡] as existing in the epithelium, the pigmentum nigrum, "cellular" tissue, and cartilage. In describing the first origin of muscle, nerve, and the crystalline lens, also, I directed attention to such centres, stating that we might hereafter see reason for thinking it not unimportant that the contents of the "primitive" cell, and those of the "secondary" cylinder, should have their origin in the nucleus; and I was particularly desirous of connecting this fact with the existence of the orifice in question. It will now be seen that I had in view the subject of *aſsimilation* now referred to.[§]

9. Such, then, were my views long since.[||] They remain unaltered; and I have the satisfaction of recognising a confirmation of them in the views just published by Dr Carpenter, which to me are the more valuable from his having formed them without any knowledge of my own.

10. Dr Carpenter directs attention to the large number of transitory cells which I had shewn in each instance to form a sort of pabulum for the central ones, and most justly adds, "Is it to be supposed that all this *cell-life* comes into existence without some decided purpose?"[¶] He considers that "the conversion of the chemical compound into the organizable

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[*] They are not in advanced stages, as it has been said, the nuclei themselves of these cells. And it is a mistake to suppose them fresh "deposits," which, however, has been done.

[†] Phil. Trans. 1842, Pl. xi. fig. 155. Ibid, 1841, Pl. xxii. figs. 110–116.

[‡] Phil. Trans. 1841, pars. 119, 120, 135, 144.

[§] Phil. Trans. 1841, par. 102.

[||] It will be seen from the preceding paragraphs that they were essentially the same in June 1841.

[¶] British and Foreign Medical Review, No. XXIX., Jan. 1. 1843, p. 270.

principle, such as mucilage into elaborated sap, or albumen into fibrin, is effected in particular situations by the vital agency of transitory cell-life."*

11. The foregoing paragraphs (1—8) will shew that I go farther. Assimilation of the substance introduced into the parietal nucleus of the cell appears to me to be *part of the process which propagates the cell. The reproduction of cells is essentially fissiparous; and it is a process of assimilation that prepares them for being cleft.*

12. I proceed to state rather more in detail, yet very briefly, the facts on which my opinion rests.

13. The orifice above mentioned, as contained in a certain part of the cell-wall, represents the situation of a highly pellucid substance, originally having little if any colour. This substance exhibits properties remarkable indeed; and as I shall have occasion constantly to refer to it in this memoir, I may be permitted, for the sake of avoiding repetition, provisionally to denominate it *hyaline*,† a term which seems unobjectionable, from its being descriptive of the appearance only. This hyaline is primogenital and formative. It appropriates to itself new matter, then divides and subdivides into globules, each of which passes through changes of the same kind. Under certain circumstances it exhibits a contractile power, and performs the motions called molecular. It is this hyaline which is the seat of fecundation in the ovum, and it is present in the large extremity of the spermatozoon; it is by successive divisions of this substance that properties descend from cell to cell, new properties being continually acquired as new influences are applied, but the original constitution of the hyaline not being lost. The main purpose for which cells are formed is to reproduce the hyaline; and this they do by effecting the assimilation which prepares it to divide. *The division of the hyaline is thus the essential part of fissiparous generation.*

14. Schleiden was the first to direct attention to what I believe to have been a globule of hyaline in his "cytoblast;" and its various appearances he has faithfully described. But

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* British and Foreign Medical Review, No. XXIX., Jan. 1. 1843, p. 271.

† A term suggested to me by Professor Owen.

as regards other points connected with this globule, Professor Schleiden and myself are not agreed.

15. Schleiden considers the substance of the "cytoblast" to be deposited *around* the globule.* According to my observations, on the contrary, a globule of the substance in question —hyaline—*appropriates to itself new matter;* the new matter is not deposited *around* the globule, but the globule, as it appropriates the new matter, becomes transformed into a disc or discoid body corresponding to the "cytoblast" of Schleiden.† A pellucid substance then begins to make its appearance within the discoid body. This pellucid substance (hyaline) is not, as supposed by Schleiden, the *identical* previously existing globule; it *results* from the appropriation of new matter by that globule, and now comes into view at a certain part of what was that globule.‡

16. It appears to me then, that the originally independent globule of hyaline is the true cell-germ,—the "cytoblast" of Schleiden representing only a stage in the formation of the cell.

17. The membrane of the cell§ is of comparatively small

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* The following is the substance of a statement made by Professor Valentin, as briefly expressing the views, on this subject of Shleiden, Schwann, Müller, Henle, and himself; and as being descriptive of the first formation of the elements of tissues:—In a fluid, says he, there are precipitated granules, which are nucleoli; *around* the nucleolus there is deposited a finely granular substance, by which there is formed the nucleus ("cytoblast"); and *around* the nucleus there is formed the membrane of the cell. The principle of formation of the nucleus *around* the nucleolus, is essentially the same as that of the cell *around* the nucleus. Valentin concludes that this process may be described by the expression, heterogeneous *circum*position.

† The terms "Cytoblast" and "Nucleus of the Cell," seem to have been used indiscriminately. It is my opinion that they are very different things, and that it is important to point out the difference. The "cytoblast" exists before the cell. The "nucleus of the cell" is sometimes that which remains of the "cytoblast" after the membrane of the cell is formed; and sometimes—the remains of the "cytoblast" having been entirely resolved into the contents of the cell—the "nucleus" is a subsequent formation in the same part. It is then the "nucleolus" enlarged, *i. e.*, the "hollow nucleus" of authors; this hollow nucleus being no other than the hyaline in the course of appropriating to itself new matter.

‡ When two or more nucleoli are present, they result from a division of the pellucid substance—hyaline—into as many parts. (Par. 13).

§ Formed, according to my observations, of the outer part of the "cytoblast," and not *around* it. (Phil. Trans., 1841, p. 209).

importance,—an envelope raised for the operation in it of the assimilative process which prepares the substance in its cavity to undergo division.

18. The following may serve as a rude sketch of this process of assimilation, and of the generation of new cells to which it leads.

19. In the first place, the cell fills with minuter cells, the germ of each of which is given off by the hyaline nucleus of the parent cell; which nucleus then appropriates to itself the result of this cell-formation,—at the same time dividing into two halves.

20. More particularly, the process (as witnessed in the ovum) appears to be as follows:—The cells, formed at the expense of the parent nucleus, are in concentric layers. The outer or first formed layer liquefies, and the second layer enlarges by imbibition and assimilation of the substance of the first. The second layer in its turn undergoes liquefaction; and now the third, enlarging, receives and assimilates the already combined substance of the first and second, and so on,—the assimilation becoming more and more complete as it advances towards the centre.

21. This, however, is the merest outline; for the cells in the concentric layers are themselves filled with other cells, which have arisen in the same manner,—in which the *same process* is going on,—and the product of which is elaborated by *their* parent cells; the latter, again, being subordinate to the first mentioned parent cell. (Par. 19).

22. The result of this many-times-repeated process is, that there is produced a mass of highly refractive globules of hyaline, which are the essential parts—the hyaline nuclei—of as many cells.* The mass requires only to be divided into two halves; a change effected by means of two cell-germs, into which the nucleus of the original parent cell divides. These two cell-germs have a central situation; they imbibe the surrounding pellucid assimilated substance—the hyaline—as a sort of pabulum; and, as they do so, become two cells, filled with the hyaline of other cells; and now the membrane of the parent cell disappears.

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* Phil. Trans., 1840, Plates XXIV., XXV., XXVI.

23. The two liberated young cells are endowed with qualities resulting from an intimate mixture of the substance previously contained in their parent cell, with that (from the seminal fluid) which their parent cell receives: a third substance being thus produced. The young cells, containing this third substance, are fertilized or undergo a sort of fecundation in their turn; become parent cells; produce another generation, which pass through like changes: and so on (Par. 6).*

24. Such appears to be the nature of the process I witnessed in the original cell, constituting the mammiferous germ, and in the cells immediately descended from it. I have since shewn that other cells present appearances denoting a similar mode of origin; though a parent cell sometimes produces *many* cells instead of two.

25. The reproduction of the cell is thus essentially fissiparous, its contents undergoing division after having been as-

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* I lately communicated to the Society the fact that I had found, and shewn to others, Spermatozoa *within* the ovum; and this after the essential part had divided into two cells. (Plate V., Fig. 1). (In one instance I counted more than twenty in a single ovum). It would thus seem that the ovum, besides being fecundated by a substance received into the part denominated by me the point of fecundation, *continues* to be influenced by the seminal fluid. I conceive that the spermatozoa may *elaborate* this fluid, in a manner comparable to that in which the red blood-discs elaborate the liquor sanguinis. (Par. 34.) Perhaps they *directly* elaborate the contents of the ovum also. (The spermatozoa which I saw within the ovum, gradually disappeared by liquefaction. I thought I saw some of them *within*, as well as between, the cells contained in the ovum). I think it possible that on the quantity of the so elaborated substance that finds its way into the germ, as well as on the degree of its elaboration, may depend the amount of resemblance between the offspring and its father.—[I have received a letter from my friend Professor Schwann, dated Loewen, 23d May 1843, informing me of the following experiment performed by him in the spring of 1842, in order to determine the influence of the spermatozoa in fecundation. Having removed a portion of the seminal fluid from the testis of a frog, and diluted the same with water, he filtered it through paper. Ova were then taken from the ovary of a frog, and treated in two ways. To some of them there was added a portion of the fluid that had passed through the paper; to others, a portion of that which had not passed through. The fluid that had passed through the paper did not fecundate, not a single ovum was developed; while that which had not passed through effected fecundation very well. "From this," says the Professor, "it follows that fecundation requires a substance which is contained in the seminal fluid, but which, not passing through the filter, is not dissolved in the fluid."]

similated to the nucleus, which nucleus, therefore, it is that is reproduced; this nucleus being a portion of the remarkable substance, hyaline, the production of which I have stated to be the main purpose for which cells are formed.

26. The changes which the hyaline undergoes, appear to be essentially the same wherever it is found, so that many of those which I am about to mention, regarding one class of objects, may be understood as essentially applicable to all.

27. I select the corpuscles of the blood as objects very easily obtained, and therefore presenting to other observers the means of testing the accuracy of my observations. It is proper, however, to remark, that no observer can learn the structure of the blood-corpuscles, who does not carefully investigate their mode of origin, and patiently follow them through all their changes.

[The observations here referred to will be found recorded in the London, Edinburgh, and Dublin Philosophical Magazine, 1st May 1843.]

34. I have no doubt that Dr Henle is right in his opinion, that the blood-discs elaborate the *liquor sanguinis*. But how is it that they do so? I apprehend it to be in the manner implied by the description just given. The red blood-discs appear to be floating centres of assimilation. They derive nourishment from the *liquor sanguinis*, which has received the chyle; giving in return, or rather resolving themselves into, hyaline.

38. I am indebted to the kindness of William Addison for a paper just published by him,* in which, after stating that the researches forming the subject of it were commenced, and many of the results committed to paper, previously to the perusal of my memoirs "On the Corpuscles of the Blood,"† the author adds, that, so far as his observations have extended, he is enabled to confirm my conclusion. This remark applies to "epithelial cells, pus-corpuscles, tubercle, tubercular infiltrations, and hepatization of the lungs."‡ These he finds to be derived from the colourless corpuscles of the blood, which with me he traces back to the nuclei of the red ones.

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* On Inflammation and Tubercle.

† Printed in the Phil. Trans. for 1840 and 1841.

‡ *Loc. Cit.* p. 4.

39. I long since performed the experiment of Müller,—that of filtering the fresh-drawn blood of frogs, and thus removing its corpuscles. After some time, a gelatinous substance was found in the filtered fluid; but I could discern no fibres in it.

Fissiparous Reproduction of the Muscular Fibril.

48. I have met with states of the muscular fibril, which, in the first place, shew that here also is to be recognised the fissiparous mode of reproduction; and, secondly, may perhaps explain the cause of the great difference between the observations of some others on this tissue and my own.

49. I saw a double spiral (Plate V. fig. 2, α)—the formed and *contractile* muscular fibril—dividing into two rows of pellucid particles of hyaline (β), apparently nuclei, that had been contained within the *substance* of the interlacing spirals (fig. 3), and were now enlarged. In another instance (fig. 2), the nuclei of each row (γ) were dividing (δ) and forming new spirals (ε). One fibril appeared thus, by fissiparous generation, to be giving origin to two.

50. The particular mode in which a row of nuclei becomes a spiral thread, I have not with certainty ascertained; and can, therefore, do no more than offer the following as probable, and as being in accordance with what I have seen elsewhere.

51. Nuclei, by elongating, form *contractile* cilia; and filaments are seen proceeding from nuclei in opposite directions.* Were filaments, thus formed by each half-nucleus (fig. 2, δ) of two adjacent rows, to assume the spiral form and interlace, and the filaments of the same row to then unite, we should have the double spiral. (The oblique position of the two rows of half-nuclei in fig. 2 δ is not undeserving of notice here.)

52. There is another subject to which I ask particular attention in connexion with the properties above mentioned (par. 13) as inherent in the hyaline—the subject of fissiparous reproduction of the Infusoria.

53. *The Infusoria compared with Cells.*†—Between the ap-

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* See Phil. Trans. 1841, Plate XXII. fig. 115, in which filaments of "cellular" tissue will be found delineated as thus forming.

† [On the subject of the generation of the Infusoria, Professor Owen remarks: —" With regard to the more common fissiparous mode, Ehrenberg has figured

pearances presented by the mammiferous germ during the passage of the ovum through the oviduct, and those met with in the young *Volvox globator* while within the parent, I find a resemblance which is very remarkable indeed, extending even to minute details. Not only do the cells of which the young *Volvox* is composed form a body resembling a mulberry (fig. 8, δ.), with a pellucid centre, but the cells gradually increase in number apparently by doubling, and at the same time diminish in their size, like the cells of the mammiferous germ, which they resemble also in being originally elliptical and flat (β, γ).

54. Some of the points of resemblance now mentioned, I recognised in the delineations of the *Volvox* given by Professor Ehrenberg;* others were noticed during some observations I have myself made on this very interesting microscopic object.

55. Ehrenberg has figured five pellucid globules in a young *Volvox* just escaped from the parent.† These, the germs of another generation, evidently resulted from division of the pellucid mass (the hyaline) visible in an earlier state (fig. 8. ε); so that here is to be recognised fissiparous generation of the kind I have described as reproducing cells.

56. On comparing the figures given by Ehrenberg of successive generations of the *Chlamidomonas* (fig. 4), with the successive groups of cells (two, four, eight, &c.) in the mammiferous ovum,‡ I cannot help believing that the process of formation is the same in both; the essential part of this process

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gradations of this spontaneous division of the organized contents of the integument in the *Gonium* and *Chlamydomonas*, which may be compared with the earliest stages of the development of the germ, as figured by Siebold in the *Strongylus* and *Medusa*, by Baer in the Frog, and by Barry in the Rabbit. Dr Martin Barry, who has discovered the very remarkable and complicated nature of this process in the mammalian ovum, was alone perhaps in the condition to fully comprehend and explain its analogy to the fissiparous generation of the polygastria, to which, in 1840, I briefly alluded; and this he has done in a paper replete with interesting generalisations, lately read before the Royal Society." *Hunterian Lectures. By Professor Owen, F.R.S., from notes taken by W. W. Cooper, M.R.C.S.*, 1843. The paper here referred to by Professor Owen, is that of which the present communication is a part.]

* Die Infusionsthierchen als volkommene Organismen. Leipzig, 1838.

† *Loc. Cit.* Tab. iv. Fig. I. 2.

‡ Researches in Embryology. Phil. Trans. 1839 and 1840.

consisting, as I shewed, in the division of a pellucid mass (hyaline) situated in the centre of each cell. And it is deserving of remark, that Ehrenberg describes his *Monas bicolor* (fig. 7.), evidently a nucleated cell, as possibly an early state of the *Chlamidomonas* just mentioned (fig. 4.)*

57. The curiously symmetrical forms of many of the *Bacillaria* appear to be due to the same division and subdivision of the pellucid nuclei or hyaline of cells.

58. The delineations of *Gonium*, *Monas vivipara* (fig. 5), and *Ophrydium*, given by the great naturalist just mentioned, afford satisfactory examples of a pellucid body dividing and subdividing like the nucleus of a cell.

59. In many other of Ehrenberg's figures of the polygastric Infusoria, the corresponding part appears to me to be denoted by a blue, red, or green colour, according as there had been added either indigo, carmine, or sap-green. This accords with what has been mentioned in a former page regarding cells—that a foreign substance becomes added and assimilated through the nucleus. Fecundation of the ovum takes place in the same manner as nutrition of the cell, and seems comparable to the nutrition of one of the Infusoria.†

60. But farther, I recognise in Ehrenberg's delineations of the Infusoria, not merely a cell-formation, but everywhere the existence of transitory or assimilative cells.

61. And farther still: the infusorial cells, like the cells of larger organisms, have their origin in globules which become discs or "cytoblasts;" these passing through stages correspond-

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* "I had often remarked," says W. Addison (*loc. cit.* p. 43), "the very great similitude of size and appearance between several forms of the polygastric animalcules, and some of the varieties of pus corpuscles. So great is this similarity, that, in many instances, it would have been difficult to distinguish the one from the other, had it not been for the voluntary and very active movements of the animalcules." The same author adds, that when a polygastric animalcule is touched by liquor potassæ, its body bursts, and liberates the particles called stomachs; from which, and from other circumstances mentioned by Addison, Dr Carpenter infers that the particles in question "are cells which float in the fluid of the body, and elaborate the materials for its nutrition, in the same manner as do those of the chyle and blood of higher animals."—(Carpenter, *loc. cit.*, p. 274.)

† The orifice in the wall of the germinal vesicle and in that of other cells appear also, in some instances, at least, to correspond to the "mouth" of the Infusoria; this "mouth" having apparently once been an orifice in the parietal nucleus of a cell.

ing to periods in the life of ordinary cells. Thus in Ehrenberg's *Monadina* are to be found, I think, the following objects:

1. Globules and discs.
2. Discs with a pellucid point.
3. The pellucid part dividing.
4. Nucleated cells.
5. The nucleus dividing, and thus giving origin to,
6. Young cells, which are seen both within and escaped from parent cells.

62. The same family, the *Monadina*, present also *ciliated* discs,—the discs being either single, or in groups radiating from a centre.

63. In 1840, I communicated to the Society the fact, that I had found blood-corpuscles revolving; and in 1841, delineated in another memoir the cilia, by means of which this takes place.* These cilia, sometimes presented by the star-like blood-corpuscle, are the elongated discs into which its nucleus or hyaline divides.

64. My observations on Spermatozoa, also communicated in 1841, shewed them to arise in the same manner.

65. It is not meant that the discs simply become sharp-pointed or elongated. So far from this, the process is very elaborate. There takes place division and sub-division, so as to produce extremely minute discs, which coalesce to form the cilia. My belief is, that it is by globules of the pellucid substance, so constantly referred to in this memoir—the hyaline—that cilia of every kind are formed; the globules first passing into discs in the manner above described (par. 15).

66. How striking the resemblance between some spermatozoa (fig. 1) and the ciliated discs in Ehrenberg's *Pandorina!* (fig. 6). The spermatozoa, as just stated, arise from division of the nucleus of a cell, *i. e.* from division of the hyaline; and I think it is scarcely less obvious that the ciliated discs of the *Pandorina* have the same mode of origin.

67. Membrane is formed by the same means as those producing cilia, namely, by division and sub-division into ex-

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* Phil. Trans. 1841, Plate XXII. figs. 104, 105. p. 226, par. 124. It was stated that the examination was made, in one instance, 18 hours, and in the other, two days after death.

tremely minute discs, which in like manner coalesce;[*] there being left here and there a centre for the origin and assimilation of new substance, in order to the thickening or other modifications of the membrane. In Ehrenberg's *Ophrydium* is to be seen, I think, a membrane forming in this manner, and dividing at one extremity into cilia, destined to surround the future "mouth." Delineations given by myself of the epithelium-cylinder[†] render it extremely probable that here the cilia arise in the same manner.

68. The division and coalescence of discs now mentioned lead to assimilation; so that the structure into which the discs have become converted, at last consists of a substance more or less like that of the cell-nucleus or hyaline itself. These remarks are applicable to other structures besides membrane,—for instance, to cartilage and fibre; and the facts above mentioned regarding muscle (par. 48–51) seem to indicate the mode in which, by means of continually enlarged and then dividing nuclei (portions of hyaline), the muscular fibril[‡] is nourished and renewed.

69. The chorion affords, perhaps, the best example I have met with, of membrane forming by the assimilative process now described.[§] For the formation of this structure, blood-cells send out processes in several directions; the processes interlace, and the pellucid nuclei of the blood-cells follow the direction of (that is to say, portions of them pass into) the interlacing processes. In short, there takes place so extended a division and sub-division of the nuclei or hyaline, that the chorion itself is scarcely less pellucid than the nuclei which form it. The albumen seems to pass through, or rather to be formed by, an assimilation of the same kind.

70. The cilia-bearing network of the parent-wall in the *Volvox* appears to have an origin not essentially differing from that which gives origin to membrane. Indeed, I think it not improbable that the membranes of many cells—for instance those of the mammiferous germ in the oviduct—may really have a structure comparable to that network of the

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[*] In some instances first forming a spiral thread.

[†] Phil. Trans. 1841, Plate XXI. figs. 96–100.

[‡] And perhaps fibre in general.

[§] See Phil. Trans. 1840, Plate XXVIII. figs. 252, 253, p. 545. par. 372. Plate xxix. figs. 6, 7, 8. p. 600.

Volvox. And it is not improbable that, revolving as no doubt the essential part of the mammiferous ovum for a short period does,* it revolves by means similar to those which produce the rotations of the *Volvox.*

71. There really seems to have been much truth in the remark long since made by Oken, that the larger animals are formed of bodies comparable to the Infusoria. The cell itself is a little organism, and cells coalesce to form a larger one.

72. The remarks made in this memoir respecting fissiparous generation, are of course intended to apply also to gemmiparous reproduction, or propagation by means of buds.†

73. *The so-called "Spontaneous Generation."—Parasites.* How do animalcules and vegetable productions arise in the infusions of organic matter? I venture to believe they may have their origin in those particles (of hyaline) which I have called the true cell-germs (par. 16.) These cell-germs, as part of the animal, or vegetable organism—for instance, in the elaborated *liquor sanguinis* (par. 34), or the descending sap—would have been developed according to the stimulus received within that organism; but now set free, each becomes developed into an independent organism, capable of propagating itself, and producing a like form, which it does in a variety of ways.

74. Is not this the mode of origin also of the Entozoa, and of all parasitic growths? If cell-germs become developed after they have left an organism, they may surely lead an independent (though parasitic) life within it,—become developed into various forms,—and propagate their species.‡

75. It is known that the various organisms, and even organs, have their *peculiar* parasites. If the view just mentioned be admitted, this is no other than what we should expect, from

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* Professor Bischoff of Heidelberg, on seeing the description given by myself (Phil. Trans. 1839, p. 355) of the "Rotatory motions of a mulberry like object in vesicles under the mucous membrane of the uterus" in the Rabbit, forthwith proceeded to an examination of the ova of this animal *as they lay in the oviduct,* and found what he calls the "yelk" in a revolving state: the rotations being produced by cilia. (Müller's Archiv, 1841, Heft 1).

† Both attached and separated.

‡ I found in the blood of a heart that had been kept for several days, moving *vibrio*-like bodies, which appeared to have been derived from the nuclei of corpuscles of the blood; and in what was apparently an ovum, escaped into the Fal-

specific peculiarities of the organisms. It is in perfect keeping, too, with the known fact, that the kind of animalcule in an infusion very much depends on the kind of organic matter decomposing in it.

76. Farther, the divisions and subdivisions of that remarkable substance, the hyaline, to which I have so constantly referred, effected as they are by a process of assimilation, incline me to believe, that even the minutest germ of the minutest disc and cell is derived from a previously existing germ. The mysterious reappearance of parental qualities in offspring, we probably shall never fully understand; but certainly the mystery is not lessened by the idea of "spontaneous generation."

LONDON, 26. 1 mo. (*January*) 1843.

Explanation of the Plate.—Plate V.

Fig. 1. Rabbit (*Lepus Cuniculus*, LINN.) Outline of an ovum of 24 hours from the middle of the Fallopian tube. Spermatozoa are seen *in its interior.* This was the case with several other ova of this rabbit. One contained more than twenty spermatozoa. *f*, "Zona pellucida." *b s*, The germ. (Par. 23, note.)

Fig. 2. Muscle from the heart (ventricle) of a frog (see pars. 48–51.). Drawn as magnified 600 diameters.

Fig. 3. Scheme shewing the situation of the nuclei in the muscular fibril (see pars. 48–51.)

Fig. 4. *Chlamidomonas Pulvisculus.*—Stages of self-division. This figure is from Professor Ehrenberg, who considers the pellucid part in the centre to be the "spermatic gland." (Par. 56.)

Fig. 5. *Monas vivipara.* α Is undergoing self-division. This figure is from Ehrenberg, who remarks respecting α: "In its interior is to be recognised the incipient division of the central spermatic gland, and three swallowed specimens of *Chlamidomonas pulvisculus;* two of which remain in one half, and one in the other." (Par. 58.)

Fig. 6. *Pandorina Morum.* From Ehrenberg, who remarks, that the contained animalcules "appear crowned." (Par. 66.)

Fig. 7. *Monas bicolor.* From Ehrenberg: "Perhaps only a state in the development of *Chlamidomonas.*" (Par. 56.)

Fig. 8. *Volvox Globator.* From Ehrenberg. "First stages in the development of a young Volvox;" α being the earliest, and ι the most advanced. (Pars. 53–55.)

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lopian tube without impregnation, I saw a body having a form somewhat the same. This body, I think, may have had its origin in the *macula germinativa* of the ovum.

An Attempt to Explain the Leading Phenomena of Glaciers.
By Professor FORBES, Sec. F.R.S., &c.*

The dilatation theory considered, and compared with observation—the gravitation theory examined—the author's theory proposed—glaciers really plastic—conditions of fluid motion—compared with those of a glacier—effect of viscosity—the veined structure of the ice a consequence of the viscous theory—illustrated by experiments—comparison of a glacier to a river—conclusion.

> "Rien ne me paraît plus clairement démontré que le mouvement progressif des glaciers vers le bas de la vallée, et rien en même temps ne me semble plus difficile à concevoir que la manière dont s'exécute ce mouvement si lent, si inégal, qui s'exécute sur des pentes différentes, sur un sol garni d'aspérités, et dans des canaux dont la largeur varie à chaque instant. C'est là, selon moi, le phénomène le moins explicable des glaciers. Marche-t-il ensemble comme un bloc de marbre sur un plan incliné? Avance-t-il par parties brisées comme les cailloux qui se suivent dans les couloirs des Montagnes? S'affaisse-t-il sur lui-même pour couler le long des pentes, comme le ferait une lave à la fois ductile et liquide? Les parties qui se détachent vers les pentes rapides suffisent-elles à imprimer du mouvement à celles qui reposent sur une surface horizontale? Je l'ignore. Peut-être encore pourrait-on dire que dans les grands froids l'eau qui remplit les nombreuses crevasses transversales du glacier venant à se congeler, prend son accroissement de volume ordinaire, pousse les parois qui la contiennent, et produit ainsi un mouvement vers le bas du canal d'écoulement."
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> RENDU, *Théorie des Glaciers*, p. 93.

I have already stated the usually received opinions as to the cause of the formation and maintenance of glaciers. We found that authors are pretty well agreed in considering that the snow which falls on the summits of the Alps becomes converted into ice by successive thaws and congelations, but that the details of the process are by no means so well understood, and that the immediate cause of the descent of these frozen masses towards the valleys has been very differently explained.

The chief theories we reduced to two; the theory of DILA-

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* From Professor Forbes' delightful volume, entitled, *Travels through the Alps of Savoy and other parts of the Pennine Chain.*

TATION and that of GRAVITATION. On the former the ice is supposed to be pressed onwards by an internal swelling of its parts, occasioned by *rapid* alternations of freezing and thawing of its parts, or rather by the continual formation of minute crevices, into which water, derived from the warmth of the sun and the action of the air on the surface, is introduced, and where it is frozen by the cold of the glacier, whose bulk it thus increases. On the theory that gravity or weight is the sole cause of glacier motion, the ice, lying on an inclined plane of rock, is supposed to slide over it, by its natural tendency to descend, aided by the action of the earth's warmth, which, on the hypothesis of De Saussure, prevents it from being frozen to the bottom.

It may be proper now to enquire shortly what light has been thrown upon these two theories by the observations detailed in a former part of this volume.

Of the facts which I have established with respect to the motion and structure of the ice of glaciers, two seem at least to be not opposed to the theory of DILATATION. I mean the *more rapid movement of the glacier at its centre*, and the *infiltration of its mass* by water permeating the capillary fissures. The former fact having been unknown to the supporters of the dilatation theory, has not been adduced by them in its favour; which it is, indeed, only thus much, that a body having a certain consistence and variability of form, when subjected to *any* pressure, whether internal or external, will yield soonest in those parts which are least retarded by friction. This fact, however, has no direct bearing on the *cause* of the pressure.

The latter fact would be entirely favourable to the theory of De Charpentier and Agassiz, could it be carried out in its consequences, in the manner which they suppose. But it is not enough that there be capillary fissures and crevices, and that these be filled with water,—*that* does not help the matter at all,—it must also be shewn that that water undergoes conversion into ice, so as to dilate it at the time, and to the extent, required for the motion. I conceive that the observations which I have made, shew such a cause of motion to be inconsistent with the phenomena; and this inconsistency is two-fold, first, from the direct evidence that, though the ice is permeated by water, yet the water freezes rarely, and to an insignificant extent; and secondly, from the motion of the glacier in its different parts, and at different times, being at variance with what must have held true upon the theory in question.

1. The water included in a glacier is rarely in a freezing condition. I need not now repeat the arguments which I have adduced to shew, that, upon every principle of the doctrine of heat, especially the doctrine of latent heat, it is impossible that the transient cold of the night should in any circumstances produce more than a superficial and most imperfect congelation,—that to suppose any thing else, would be to suppose in a glacier an indefinite supply of cold,* contrary to first principles, and to direct observations with the thermometer on the temperature of the ice, which has been found by M. Agassiz himself to be constantly, and at all depths, within a fraction of a degree of 32°. But besides this, the most direct observation shews, that the nocturnal congelation, which is so visible at the surface, drying up the streamlets of water, and glazing the ice with a slippery crust, extends to but the most trifling depth into the mass of the glacier. This is so evident, upon consideration, that when fairly placed before him, M. de Charpentier has been obliged to abandon the idea that the diurnal variations of temperature produce any effect. In truth, there is positive evidence that no internal congelation takes place during the summer season, when the motion is most rapid, and when, therefore, the cause of motion must be most energetic. Of this I will give one striking example.

Towards the end of September 1842, when a premature winter had covered the Mer de Glace with snow, and lowered the temperature of the air to 20° Fahrenheit, I had occasion to make an expedition over nearly its whole extent, in the direction of the Glacier de Léchaud, in order to observe the marks which had been placed in that direction, and to determine the motion of the higher parts of the ice. The excursion promised to be far from agreeable. The sky was lowering when we started from the Montanvert, and it soon began to snow, and continued to do so with little intermission during the day. The Mer de Glace had been covered with snow for a week; at the Montanvert, to a depth of six inches, but in its higher parts of not less than a foot and a half. I was not sorry, however, to have an opportunity of ascertaining the conditions of the ice, under circumstances so critical for the theory of dilatation, for now, if at any time, the freezing and expansive effects of cold ought to be visible, the ice having been completely saturated by the preceding wet weather, and, it might be supposed, effectually cooled by five days of frost. As the walk promised to be laborious, if not difficult, owing to the thick coating of snow, I took with me David Couttet of the Montanvert, and Auguste Balmat, as usual, with the instruments and provisions. We started in a lowering morning at half-past six, and in less than an hour it began to snow, with a drifting wind, though fortunately without cold. To most persons, the journey would have been an alarming one, but we were all three so intimately acquainted with the surface of the ice, and the direction of the moraines, that we had no fear of losing ourselves. It required, however, all Auguste's intimate knowledge of the glacier to keep us clear of dangerous crevasses and holes; for the snow was often knee-deep, and the glacier and moraines alike filled with innumerable pit-falls. We crossed the moraines, as usual, near the Moulins, and visited the stations B 1 and C. We then kept nearly under the ice-fall of the Glacier du Taléfre, and reached with precaution the higher glacier of Léchaud, on our way to station E, where I anxiously wished to make an observation of the progress of the glacier. But now the bad weather increased so much, that we were glad to get behind a great stone and eat our breakfast, waiting for a favourable change. The wind blew in strong gusts from the Grande Jorasse, tossing the snow about so as to render all objects at a distance undistinguishable, thus threatening to make our expedition ineffectual, for the rock called the Capucin du Tacul, which was my index for the bearings on the glacier, from station E, was hopelessly invisible. After some delay, the storm abated, and the Pierre de Béranger, whose azimuth I had fortunately taken as a check, shewed itself. We therefore advanced up the glacier, but again the storm thickened, and as we got to the foot of the rock on which station E was fixed, David Couttet (who had hitherto been the chief encourager of the expedition) said quietly, " Nous allons faire une bêtise," and proposed to return, for we were half blinded by the snow. I begged, however, that we might at least stop and take shelter as before. We did so, and profiting by a few minutes' pause in the drift, I fitted up my theodolite, and took an observation of the motion of the glacier since my last visit with due care and deliberation. We then returned nearly as we had come, fortunately without accident, and reached the Montanvert after nine hours' absence. What struck me most in this expedition was, that even at the highest station, which is 7900 feet above the sea, and in this severe weather, the ice, far from being frozen to a great depth, appeared charged with water as usual, except at the surface. The stick which marked the point of the glacier observed, and which I expected to find firmly frozen into its place, was *standing in water* in its hole in the ice, and of course quite loose. The *surface* of the glacier generally was dry,—there was not a rill of water in the Moulins, or elsewhere : yet the congelation had scarcely penetrated at all. Couttet and Balmat were all the time afraid of treading into a watery hole, and thus getting their feet frozen, an accident which I thought very unlikely to happen ; but they both did get their feet wet in the course of the day. Hence there can be no doubt, that, as Couttet very distinctly expressed it, the snowy covering kept the glacier warm, just as it does the ground, and that the cold penetrates extremely slowly even when winter arrives. I may add, that near the Tacul I found no difficulty in obtaining a draught of water by breaking the crust of ice formed on a pool in the glacier under a stone. It was on this excursion that I observed the blue colour of snow, previously mentioned, which was most distinctly perceptible by transmitted light, whenever the snow was pierced by a stick to a depth of six inches or more. It was at one part of the glacier that this was most evident, which I attributed to the particular degree of aggregation which it had there, neither very dry nor very moist.

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* This argument has been well put by M. Elie de Beaumont, with his accustomed clearness.

From the incidents just related, I think it seems to be demonstrated beyond a doubt, that, at least, any *transient* impression of cold is quite incapable of converting the infiltrated water into ice at any depth in the glacier.

2. At the same time that the preceding observations were made, the rate of motion of the glacier was carefully observed; for I concluded, as a matter of certainty, that, if the dilatation theory were true, a sudden frost succeeding wet weather must inevitably cause the glacier to advance far more rapidly than in summer, or, indeed, at any other season; for there could never possibly be more water to be frozen, nor could cold ever act with more energy than at the time in question. What the facts were, we have already seen in the seventh chapter, where it appears, both from the tables and figures, that the progress of the glacier was retarded during the cold weather which prevailed from the 20th to the 25th September, and that it readvanced when the thaw had taken place some days later.

3. The motion of the glaciers during winter, established in the same chapter, is directly contrary to the conclusions invariably drawn by the glacier theorists from their supposed immobility; since they consider, that, while the glacier is completely frozen, and has no alternations of congelation and thaw, there can be no dilatation.

4. My experiments shew, that the motion of a glacier during the day and night is sensibly uniform, which is contrary to the same view.

5. The rate of motion of the glacier at different parts of its length has been shewn to be by no means such as the expansion of an elongated body, supported at one end, and pushed along its bed, would occasion.

6. The advocates of the theory of dilatation have rightly maintained, as a consequence of the theory, that the ice will expand in *all* directions, and consequently upwards, that being the direction in which the resistance is least of all. They thence conclude, that, whilst the ice wastes by melting at the surface, the surface will be raised by the inflation of the interior mass by the expansion of freezing water, and that its absolute level will thus be maintained, or will even rise, notwithstanding the daily waste. They profess to have made

experiments which confirm this view; but I have already stated, that my own are entirely at variance with it, the absolute level of the ice lowering with great rapidity during the season of most rapid motion; a conclusion which is entirely confirmed by the observations of MM. Martins and Bravais, lately published.*

On these, amongst other grounds deduced from direct observation, I consider the dilatation theory maintained by Scheuchzer, De Charpentier, and Agassiz, as untenable.

In the next place, let us consider the sliding theory of Gruner and De Saussure.

As I understand the GRAVITATION theory, it supposes the mass of the glacier to be a *rigid* one, sliding over its trough or bed in the manner of solid bodies, assisted, it may be, by the melting of the ice in contact with the soil, which possesses a proper heat of its own, and which lubricates in some degree the slope, as grease or soap does when interposed between a sliding body and an inclined plane. It is only in so far as the theory is considered as applicable to a rigid body, that I have objections to state to it.

1. In the case of the greater number of extensive glaciers, there are notable contractions and enlargements of the channel or bed down which they are urged. Let any one glance at the Mer de Glace, and see two extensive glaciers meeting at the Tacul, forming a vast basin or pool, from which the only outlet has a less breadth than the narrowest of the tributaries; the idea of *sliding*, in the common legitimate sense of the word, is wholly out of the question.

2. We have already seen that the ice does not move as a solid body,—that it does not slide down with uniformity in different parts of its section,—that the sides, which might be imagined to be most completely detached from their rocky walls during summer, move slowest, and are, as it were, dragged down by the central parts. All this is consistent with motion due to weight or gravitation; but not with the sliding of a rigid mass over its bed.

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* *Annales des Sciences Géologiques, par Rivière,* 1842.

3. The inclination of the bed is seldom such as to render the overcoming of such obstacles as the elbows and prominences, contractions and irregularities of the bed of glaciers, even conceivable, being, on an average of the entire Mer de Glace, only 9°, a slope practicable for loaded carts; but the greater part of the surface inclines less than 5°, which is below the steepest slope on the great highway of the Simplon, an artillery road.

4. It has been convincingly proved by me, that the motion of the glacier varies not only from one season to another, but that it has definite (though continuous) changes of motion, simultaneous throughout the whole, or a great part of its extent, and therefore due to some general external change. This change has been shewn to be principally or solely the effect of the temperature of the air, and the condition of wetness or dryness of the ice. In order to reconcile this to the sliding theory, it should be shewn, that the disengagement of the glacier from its bed, depends on the kind of weather which affects its surface and temperature. In no part of the summer is the glacier actually frozen to its lateral walls; the difference, then, must be due to the action of the earth's heat, in gradually melting away the irregularities of the interior surface of the ice, in contact with the rocky bed on which it reposes. I have already said, that I consider such an influence of the proper heat of the earth to be distinctly included in De Saussure's theory, as it has been stated by himself, and understood by his successors.* It was, however, suggested to me very distinctly by M. Studer last summer, as not inconsistent with a motion by gravity without acceleration; and I admit the ingenuity of the thought, which, as it will be seen in the sequel, I am disposed to allow, may be one *way* of glacier motion, though not exactly the *cause* of it. The same thought was afterwards suggested to me by Sir John Herschel, and more lately I have seen it stated, that Mr Hopkins

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* Any one who carefully reads De Saussure's § 535 in connection with § 533, will be convinced that he gives all due weight (we should be inclined to say *more* than due weight) to the effects of subterranean heat in *detaching* the ice from its bed, *lubricating* it on its bed, and even *elevating* it over obstacles by the hydrostatic pressure of confined water.

of Cambridge, the author of an ingenious pamphlet on the theory of glacier motion, has illustrated it by experiment.* But this is an effect which must remain nearly the same at all seasons, being due to the constant flow of heat from the interior.

5. The flow of heat from the interior is so *very* trifling that it may be doubted whether it is adequate to produce the particular effect of wearing off the prominences of the descending ice, or of moulding it to the form of the channel. In order to do so to any effectual extent, it would be necessary that prominences of many feet or yards in extent should be melted away in a moderately short space of time. Now, what is the fact? M. de Beaumont has estimated,† by the theory of Fourier, from the observations of Arago on the earth's temperature, that the quantity of central heat which reaches the surface of the earth, is capable of melting 6½ millimetres of ice, or exactly *a quarter of an English inch in the space of a year.* Now, even admitting (as I think we may), that if the surface of the earth were covered with ice, the flow of heat would be somewhat greater; still it must be admitted to be incapable of disposing of portions of ice insignificant compared to the inequalities which oppose its downward progress.

6. This small quantity of heat is not always applied (as Professor Bischoff‡ and M. Elie de Beaumont have justly remarked) to *melt* the ice of glaciers. Below 32° it will simply tend to raise the temperature of the ice in contact with the soil, and powerfully adhering to it. The almost *pendant* glaciers of the second order, which are seen only at great heights, those, for instance, on the precipices of the Mont Mallet, must remain permanently frozen to the rock. Nevertheless, they do actually descend over it, for they continually break off in fresh avalanches. This is a fact which neither the theory of dilatation nor that of gravity, as commonly stated, is capable of explaining.

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* Since writing the above I have been indebted to Mr Hopkins for a farther statement of his views.

† *Annales des Sciences Géologiques, par Rivière,* 1842.

‡ *Wärmelehre,* p. 101, &c.

After the detailed though scattered deductions which have been made in the course of this work, from observations on the Movement and Structure of Glaciers, as to the cause of these phenomena, little remains to be done but to gather together the fragments of a theory for which I have endeavoured gradually to prepare the reader, and by stating it in a somewhat more connected and precise form, whilst I shall no doubt make its incompleteness more apparent, I may also hope that the candid reader will find a general consistency in the whole, which, if it does not command his unhesitating assent to the theory proposed, may induce him to consider it as not unworthy of being farther entertained.

My theory of glacier Motion then is this:—A GLACIER IS AN IMPERFECT FLUID, OR A VISCOUS BODY, WHICH IS URGED DOWN SLOPES OF A CERTAIN INCLINATION BY THE MUTUAL PRESSURE OF ITS PARTS.

The sort of consistency to which we refer may be illustrated by that of a moderately thick mortar, or of the contents of a tar-barrel, poured into a sloping channel. Either of these substances, without actually assuming a level surface, will *tend* to do so. They will descend with different degrees of velocity, depending on the *pressure* to which they are respectively subjected,—the *friction* occasioned by the nature of the channel or surface over which they move,—and the *viscosity*, or mutual adhesiveness, of the particles of the semifluid, which prevents each from taking its own course, but subjects all to a mutual constraint. To determine completely the motion of such a semifluid is a most arduous, or rather, in our present state of knowledge, an impracticable investigation. Instead, therefore, of aiming at a cumbrous mathematical precision, where the first data required for calculation are themselves unknown with any kind of numerical exactness, I shall endeavour to keep generally in view such plain mechanical principles as are, for the most part, sufficient to enable us to judge of the comparability of the facts of Glacier motion with the conditions of viscous or semifluid substances.

That Glaciers are semifluids is not an absurdity.

The quantity of viscidity, or imperfect mobility in the particles of fluids, may have every conceivable variation; the extremes are perfect fluidity on the one hand, and perfect rigidity on the other. A good example is seen in the process of consolidation of common plaster of Paris, which, from a consistency not thicker than that of milk, gradually assumes the solid state, through every possible intermediate gradation. Even water is not quite mobile; it does not run through capillary tubes; and a certain inclination or *fall* is necessary to make it *flow*. This may be roughly taken as an index of the quality of viscidity in a body. Water will run freely on a slope of 6 inches in a mile, or a fall of 1-10,000 part,[*] another fluid might require a fall of 1 in 1000; whilst many bodies may be heaped up to an angle of several degrees before their parts begin to slide over one another.

Thus, a substance apparently solid may, under great pressure, begin to yield; yet that yielding, or sliding of the parts over one another, may be quite imperceptible upon the small scale, or under any but enormous pressure. A column of the body itself is the source of the pressure of which we have now to speak.

Even if the ice of glaciers were admitted to be of a nature perfectly inflexible, so far as we can make any attempt to bend it by artificial force, it would not at all follow that such ice is rigid, when it is acted on by a column of its own material several hundred feet in height. Pure fluid pressure, or what is commonly called hydrostatical pressure, depends not at all for its energy upon the *slope* of the fluid, but merely upon the *difference of level* of the two connected parts or ends of the mass under consideration. If the body be only semifluid, this will no longer be the case; at least the pressure communicated from one portion (say of a sloping canal) to the other, will not be the *whole* pressure of a vertical column of the material, equal in height to the difference of level of the parts of the fluid considered; the consistency or mutual support of the parts opposes a certain resistance to the pressure, and prevents its indefinite transmission. It must be recollected, that, in the case of glaciers, the pressing columns are enormous, the origin and termination of many of the largest having not less than 4000 feet of difference of level; were they, therefore, perfectly fluid, or suddenly converted into water, the lower end would begin to move with the enormous velocity of 506 feet a second, or would move over 44 *millions of feet* in 24 hours. Now, the velocity of the Mer de Glace is only about 2 feet in that time,—a difference so enormous that the fluidity of a glacier compared to water will not appear so preposterous as it might at first sight do, considering the small degree of transmitted pressure required to be effectual.

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[*] According to Dubuat (Hydraulique, tom. i., p. 64. Edit. 1816) at a slope a great deal lower; but its exact value does not now concern us.

Again, it has been attempted to shew that a glacier is not coherent ice, but is a granular compound of ice and water, possessing, under certain circumstances, especially when much saturated with moisture, a rude flexibility sensible even to the hand.

Farther, it has been shewn that the glacier *does* fall together and choke its own crevasses with its plastic substance.

When a glacier passes from a narrow gorge into a wide valley, it spreads itself, in accommodation to its new circumstances, as a viscous substance would do, and when embayed between rocks, it finds its outlet through a narrower channel than that by which it entered. This remarkable feature of Glacier Motion, already several times adverted to, had not been brought prominently forward, until stated by M. Rendu, now Bishop of Annecy, who has described it very clearly in these words,—" Il y à une foule de faits qui sembleraient faire croire que la substance des glaciers jouit d'une espèce de ductilité qui lui permet de se modéler sur la localité qu'elle occupe, de s'amincir, de se rétrécir, et de s'étendre, comme le ferait une pâte molle. Cependant, quand on agit sur un morceau de glace, qu'on le frappe, on lui trouve une rigidité qui est en opposition directe avec les apparences dont nous venons de parler. Peut-être que les expériences faites sur de plus grandes masses donneraient d'autres résultats."*

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* Theorie des Glaciers de la Savoie, p. 84. Whilst I am anxious to shew how far the sagacious views of M. Rendu coincide with, as they also preceded my own, it is fair to mention, that all my experiments were made, and indeed by far the

Now, it is by observations on the glacier itself that we can best make experiments on *great masses* of ice, as here suggested.

The Motion of a Glacier resembles that of a Viscous Fluid.

All experimental philosophers are agreed as to the facts, that a fluid like water, heavy and slightly *viscid*, moves down an inclined plane or canal, with a velocity which varies according to the slope, and which varies also from point to point of the section of the stream. The part of the stream which moves fastest is the surface, and especially the *central part* of the surface. The velocity of motion diminishes *on* the surface from the centre to the sides, and *from* the surface towards the bottom.

The cause of these variations is admitted to be the *friction* of the sides and bottom of the canal or bed, which retards the fluid particles immediately in contact with them, and the adhesion of these particles to their neighbours, that is, their *viscosity*, communicates this retardation by certain gradations, which are not correctly known, to the interior mass of the fluid. Hence,—

I. The centre and top of the stream move faster than the sides and bottom, especially if *the friction of the fluid particles over one another be less than their friction against the sides of the canal.* If this be not the case—if the friction of the contained mass against the containing or supporting walls be *less* than the friction which exists amongst its own particles, the mass will *slide out of its bed*, and will so far act as a solid body. If it have a certain mobility amongst its own particles, it will, whilst sliding over its bed, alter, at the same time, the relative position of its own particles;—it will move partly as a solid, partly as a fluid. We may then fairly call it a *semifluid* or a *semisolid.*

II. From this it also evidently appears, that the greater the viscosity of the fluid, the farther will the lateral and funda-

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greater part of the present volume was written, before I succeeded in obtaining access to M. Rendu's work, in the 10th volume of the *Memoirs of the Academy of Chambéry*, which I owe at length to the kindness of the right reverend author.

mental retardations be communicated towards its centre, and the general velocity of the stream will be more nearly regulated by the limit of the mobility of its parts.

III. In every case, the greatest variation of velocity of such a stream will take place near the sides and bottom, whilst the higher and the central parts will move most nearly together.[*] The position of any particle moving with the mean velocity of the entire stream, has not, I believe, been determined; but Dubuat has practically found this singularly simple result, that the velocity of the top and bottom of a stream being known, the mean velocity of the entire stream is the arithmetical mean of these two velocities.

IV. The difference of the velocities of a stream at the top and bottom, depends upon the actual velocity of the stream, and increases as that velocity increases. The rate of increase appears to be as the square root of the velocity, and is independent of the depth.[†]

V. The velocity of the water in a stream increases with its declivity. If the bed of a river be highly inclined, the water flows rapidly; and again, if the embaying of a river by a strait accumulates the water above, there its declivity will be diminished.

VI. If any circumstance causes the viscosity or consistence of a fluid to vary, all these phenomena will vary proportionally. Thus, warm water is less viscid than cold, and a vessel will be sooner emptied through a narrow aperture the higher the temperature of the liquid.[‡]

Now, in all these respects, we have an exact analogy with the *facts* of motion of a glacier, as observed on the Mer de Glace.

First, We have seen that the centre of the glacier moves faster than the sides. We have not indeed extended the proof to the top and bottom of the ice-stream, for it seems difficult to make this experiment in a satisfactory manner. In the case of a glacier 600 feet deep, the upper hundred feet will move nearly uniformly, on the principles already mentioned; hence, crevasses, formed from year to year, will not incline sensibly forwards on this account, especially as the action of trickling water is to maintain the verticality of the sides. I conceive that this is a perfectly sufficient answer to an objection which, at one time, I myself urged against the hypothesis of the glacier moving most rapidly. Of the fact, I entertain no doubt, though I see much difficulty in obtaining a satisfactory proof of it.

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[*] A slight consideration will shew, that this might naturally be anticipated. yet some eminent writers have supposed the velocity to increase *uniformly* from the bottom to the surface of a stream. The doctrine of the text is fully confirmed by direct experiments upon the river Rhine, quoted in Mr Rennie's *Report on Hydraulics*, Part II., *British Association Report*, 1834, p. 467.

[†] DUBUAT, Art. 37, 49, 65.

[‡] Ibid. Art. 3.

I have no doubt that glaciers slide over their beds, as well as that the particles of ice rub over one another, and change their mutual positions; but I maintain, that the former motion is caused by the latter, and that the motion impressed by gravity upon the superficial and central parts of a glacier (especially near its lower end) pull the lateral and inferior parts along with them. One proof, if I mistake not, of such an action is, that a deep current of water will flow under a smaller declivity than a shallow one of the same fluid.* And this consideration derives no slight confirmation in its application to glaciers, from a circumstance mentioned by M. Elie de Beaumont, which is so true, that one wonders it has not been more insisted on,—namely, that a glacier, where it descends into a valley, is like a body pulled asunder or stretched, and not like a body forced on by superior pressure alone.

Secondly, We have already seen how enormous would be the velocity of a glacier if suddenly converted into a fluid, and how prodigious a force is absorbed, as it were, by the consistency or solidity of the ice. The moderate, though marked difference, found between the lateral and central velocity of a glacier is in conformity to the second principle stated above, that the retardation due to friction will be more completely distributed over the whole section in proportion as the matter is less yielding.

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* It is well known that the mean hydraulic depth, or the ratio of the section of a stream to the perimeter of contact with its bed, is the most important element (together with the declivity) in determining its velocity, or the effectual moving force which acts upon it. Now, in the case of common friction, that of a solid body, neither the absolute nor the relative depth of the sliding body can have any influence in determining its motion.

Thirdly, The chief variation of velocity is near the sides.

Fourthly, We have found a most remarkable confirmation of Dubuat's principle, that the amount of lateral retardation depends upon the actual velocity of the stream under experiment; whether we consider different points of the glacier, or the same point at different times. *

Fifthly, The glacier, we have seen, like a stream, has its still pools and its rapids. Where it is embayed by rocks, it accumulates—*its declivity diminishes, and its velocity at the same time*;—when it passes down a steep, or issues by a narrow outlet, its velocity increases.

The *central* velocities of the lower, middle, and higher regions of the Mer de Glace are—

1.398	.574	.925

And if we divide the length of the glacier into three parts, we shall find something like these numbers for its declivity†—

15°	4½°	8°

Lastly, When the semifluid ice inclines to solidity during a frost, its motion is checked; if its fluidity is increased by a thaw, the motion is instantly accelerated. Its motion is greater in summer than in winter, because the fluidity is more complete at the former than at the latter time. The motion does not cease in winter, because the winter's cold penetrates the ice as it does the ground, only to a limited extent. It is greater in hot weather than in cold, because the sun's heat affords water to saturate the crevices: but the proportion of velocity does not follow the proportion of heat, because any cause, such as the melting of a coating of snow by a sudden thaw, as in the end of September 1842, produces the same effect as great heat would do. Also, whatever cause accelerates the movement of the centre of the ice increases the difference of central and lateral motion.

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* Une chose étonnante, c'est que ni la grandeur du lit, ni celle de la pente n'influent en rien sur le rapport des différentes vitesses dout nous parlons, *tant que les vitesses moyennes restent les mêmes* où celle de la surface est constante.—Dubuat, Art. 65.

† These numbers do not express the actual slopes at the points where the velocities were measured, but the slope of the inferior, middle, and superior regions of the glacier.

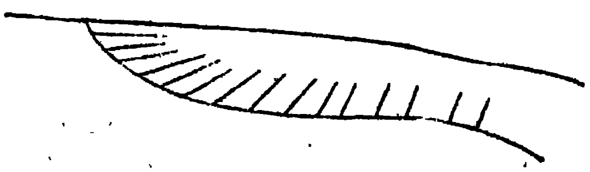
The Veined Structure of the Ice is a consequence of the Viscous Theory.

We have now to complete what was partly said in Chapter VIII., where we endeavoured to illustrate the phenomena of the veined or ribboned structure of the ice, and to explain its cause.

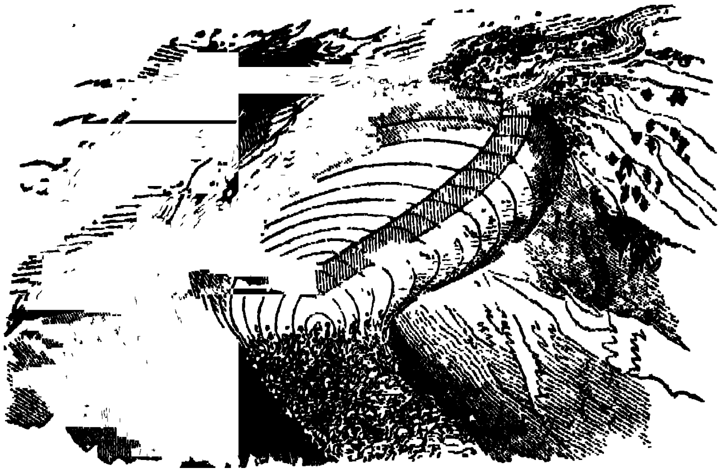
This structure we have seen to consist in the recurrence of alternations of blue and white, or compact and aërated ice in a glacier, resembling the veins in chalcedony, the parts being thin and delicately subdivided.

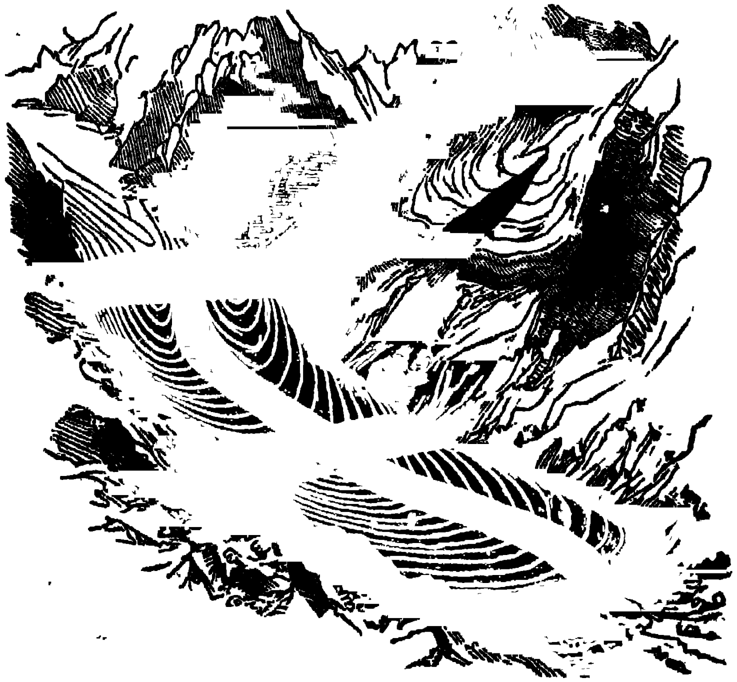
We have found that this structure has all the appearance of being due to the formation of fissures in the aërated ice or consolidated névé, which fissures having been filled with water drained from the glacier, and frozen during winter, have produced the compact blue bands.

We have farther found that this ribboned structure follows a very peculiar course in the interior of the ice, of which the general type is the appearance of a succession of oval waves on the surface, passing into hyperbolas with the greater axis directed along the glacier. That this structure is also developed throughout the thickness of a glacier, as well as from the centre to the side, and that the structural surfaces are twisted round in such a manner that the *frontal dip*, as we have called it, of the veins, as exhibited on a vertical plane cutting the axis of a glacier occurs at a small angle at its lower extremity, and increases rapidly as we advance towards the origin of the glacier, as shewn in the accompanying figure.



We have also considered glaciers generally as of three kinds, which, having a common structure, yet exhibit it in different forms or modifications. These three glacier forms may be termed the *canal-shaped*, the *oval*, and the glaciers of the *second order*. The annexed figures (one of which has been already used) shew by views with ideal sections of such glaciers, the manner in which the structural surfaces traverse the mass of the ice. The first figure shews the conoidal structure of a glacier of the oval kind. The second shews this





drawn out, as it were, into a canal-shaped glacier. On the right hand, in the upper part of the figure, a small glacier of the second order is shewn, where it appears that its structure consists of a series of superimposed shells, nearly parallel to the soil, which might easily be confounded with the annual layers of the névé.

All these structures I explain on the common principle of the difference of velocity of the higher and lower, as well as of the central and lateral parts of the ice; for wherever the parts of a stream, whether liquid or semi-solid, move with different velocities, there must be a force applied to separate them from one another.

But hear Dubuat, an eminent hydrostatical writer. Speaking of ordinary rivers, he says, "La viscosité de l'eau ou l'adhérence que ses particules ont entre elles, occasionne une résistance très-petite, mais finie, qui s'oppose sans cesse à leur séparation : or, il ne peut y avoir de mouvemente uniforme dans l'eau, sans que ses filets ne prennent différentes vîtesses, selon qu'ils sont plus ou moins proches de la paroi, qui retarde et rende uniforme le movement de toute la masse. Cette inégalité do vîtesses ne peut avoir lieu sans une séparation mutuelle des parties contiguës. La viscosité, ou, si l'on veut, la force avec laquelles ces parties s'attirent, s'oppose à cette séparation; il faut donc qu'un y ait constamment une partie de la force accéleratrice destinée à vaincre cette résistance; et lorsque la force accélératrice est assez petite pour lui être seulement égale, le mouvement doit cesser, quoique la pente soit finie. S'il existait un fluide dont les parties n'eussent aucune adhérence entre elles, la plus petite pente possible suffirait pour lui imprimer un mouvement; mais les différents liquides connus éprouvant plus ou moins l'effet de la viscosité, la pente à laquelle ils commencent à couler est d'autant plus grande que l'adhérence de leurs parties les éloigne moins de la nature des solides."*

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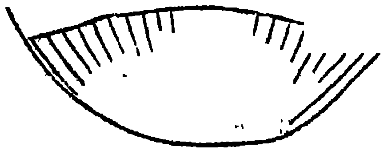
* DUBUAT, i. 58.

From this we might expect that we should have a separation of the ice particles, a rupture or fissuring of the substance of the glacier every where parallel to the resisting walls or bottom, producing a cross section, as in the annexed figure.



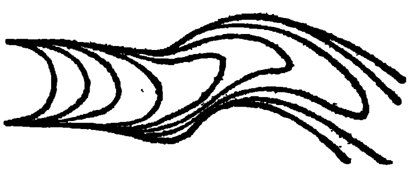
This is what actually takes place, and especially in glaciers of the second order, where the retardation being almost entirely due to the action of the bottom, the successive layers slip over one another with increasing velocity towards the surface.

But the question arises, how does this action produce the *frontal dip* of the figure at page 237? Why does not the canal-shaped glacier present a series of trough-shaped layers, as in the figure above, whose bottom remains parallel to the bottom or bed of the glacier? The reason appears to be this. The fluid is urged on (in the centre of the glacier especially) by its own *weight.* It is falling down an inclined plane by the force of gravity. It is, however, prevented from moving by the intense friction of the whole of the lower part of the glacier upon its bed. If the glacier be solid, there can be no motion, unless there be sufficient force to overcome this friction; and this we have seen to be one great (and we think insurmountable) difficulty, both of the hypothesis of De Saussure and that of De Charpentier. But the semifluid has another mode of progression,—the pressure *may* not overcome the friction of the bed, or else the fluid pressure at the lower end *may* be dragging the whole glacier over its bed, *that* is immaterial; but any particle in a fluid or semifluid mass, urged by a force from above, does not necessarily move in the direction in which the force impels it, it moves *diagonally*; forwards, in consequence of the impulse; upwards, in consequence of the resistance directly in front. Hence a series of surfaces of separation shaped (to use familiar illustrations) somewhat like the mouth of a coal-scuttle, or of a sugar-scoop, will rise towards the surface, varied

ture by the law of velocity of the different layers of

er. Near the head or origin of the glacier, where the resistance in front is *enormous*, the tendency of the *separation planes*, which are those of apparent cleavage, will be very highly inclined. As the lower end of the glacier is approached, the resistance continually diminishes, the line of least resistance becomes more and more nearly horizontal; and finally, when the lower end of the glacier is reached, the planes fall away altogether, and the upper layers roll over the lower ones, now wholly unsupported. Such we have seen to be the actual phenomena of the Mer de Glace.

Imagine a long narrow trough or canal stopped at both ends, and filled to a considerable depth with treacle, honey, tar, or any such viscid fluid. Imagine one end of the trough to give way, the bottom still remaining horizontal, if the friction of the fluid against the bottom be greater than the friction against its own particles, the upper strata will roll over the lower ones, and protrude in a convex slope, which will be propagated backwards towards the other or closed end of the trough. Had the matter been quite fluid, the whole would have run out and spread itself on a level; as it is, it assumes precisely the conditions which we suppose to exist in a glacier. The greatest disturbance or maximum separation of the parts takes place at the lower end, and there (the retardation at the sides being proportional to the *absolute* velocity) the separation will be most violent, and the loops on the surface will be most elongated. Near the origin the declivity is less, and the loops are more transverse. This is true of the glacier.



Now, let the trough be a little inclined, so as to aid the gravitating force derived from the mere depth of the fluid. Each particle will be urged on by a force due to the slope, diminished by the resistances opposed to it. The particles near the lower termination of the stream have no resistances, except their attachment to those behind them; they therefore roll straight on; but those in the middle of the glacier will easier raise the weight of a certain superincumbent stratum of ice, than push the entire glacier before them; they may do part of both, but will undoubtedly rise towards the surface, and thus *slide upwards and forwards* over the particles immediately in advance.

Though I am not aware that this form of fluid motion has been pointed out, its existence is scarcely to be doubted from very ordinary mechanical considerations, and several obvious phenomena also indicate it. Thus, such a viscid stream as we have supposed, be it tar, mortar, treacle, glue, plaster of Paris, slag, or cast metal, *invariably* presents *wrinkles*, or curvilinear arrangements of the floating matter, accompanied by a crumpling or inequality of the surface. These inequalities, these lines indicative of motion, which were the very first indications which led me, when studying the "dirt bands," to discover their nature, must be due to *inequality* of motion, and to no other cause. It is vain to look for any original linear or tabular arrangement of the particles when a fluid is poured from a ladle into a sloping channel, and afterwards becomes modified into the curves we have described. In the scum or froth on a sluggish current of water, there was no original arrangement of particles transverse to the stream, which has become *deformed* into the elongated loops in which the bubbles arrange themselves, as it were spontaneously, during their progress. These curves are the direct result of the unequal mutual pressures of the particles; and the whole phenomena, in the case of any of the semifluids I have mentioned, are such as—combined with the evidence which I have given, that the motion of a glacier is actually such as I have described that of a viscid fluid to be—can leave, I think, no reasonable doubt, *that the crevices formed by the forced separation of a half rigid mass, whose parts are compelled to move with different velocities, becoming infiltrated with water, and frozen during winter, produce the bands which we have described.*[*]

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[*] The wave-like figures of floating matter on a sluggish surface are not at all to be confounded with the actual direction of motion of the fluid particles. They are curves of *differential velocity* merely, and are always most perceptible near the *sides* of the stream, where the variations of velocity are greatest. A stream, like a mill-race, covered with saw-dust, will shew these linear markings inclining towards the centre of the stream; but the motion of any floating body, as a bit of cork, is sensibly parallel to the sides. I have proved the same thing by

I have succeded in illustrating this theory by constructing models of a viscid material (a mixture of plaster and glue, which does not *set* readily), poured down irregular channels,

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actually performing the experiment suggested in the text, of pouring plaster of Paris and glue into a narrow rectangular box, and sluicing it up by a bit of wood, removable at pleasure. The surface of the viscid mass was then strewed, whilst level, with a coloured powder, and the sluice withdrawn. The liquid flowed exactly as I have described, and the colouring matter was drawn out into threads, precisely resembling, on a minute scale, in delicacy and continuity, the veined appearance of the glacier surface. The explanation appears to be this;—that the velocity of the central strata tends to pull the lateral strata towards the centre, as well as parallel to the length of the glacier; this produces a slight lateral as well as longitudinal discontinuity, for the actual motion of the side strata towards the centre is exceedingly small, and (as the phenomena of moraines tell us, which act like the floating cork in the experiment above described) does not sensibly disturb the parallelism of motion of the parts of the ice. In short, the internal movements are of an order so inferior to the general movement of the stream, that they may probably be left out of account in describing the general movement, although by the fissured structure which they induce, they have sufficient evidence of their existence. But if the slope and consequent hydrostatic pressure be great, the movement towards the centre *may*, as supposed in the text, be of an order to modify appreciably the direction of movement of a particle. In an ordinary liquid like water, the direction of the ripple marks, occasioned by the friction of a stream in proceeding from a wider to a narrower channel, is an example of the same thing, namely, lines of maximum mutual friction of the particles against one another. They converge rapidly towards the centre of the stream, whilst the motion of the fluid, indicated by a floating body, deviates but little from the direction of the axis of the channel.

The same view, *mutatis mutandis*, explains the frontal dip and conchoidal form of the bands between the top and bottom of the glacier. There is here also a *drag* acting from the upper to the lower strata, and fissures are produced in consequence of the sluggish lower strata refusing to follow the swifter upper ones. This may also be a quantity of an order so inferior to the actual rate of motion of the ice, as to make it inappreciable by direct experiment.

The experiment, on a model described in this note, is more strictly analogous to the glacier phenomena, than those of a more striking kind described in the next page, where the succession of colours naturally gives to the mind the impression of a primitive structure near the origin of the glacier, which is mechanically *deformed* into these conchoidal surfaces. They strikingly recall, however, this important fact, that the direction of maximum distension of the particles must be, not parallel to the length of the glacier, but in the direction of the branches of these elongated loops, since their elongation is the simple result of the mechanical tension to which they are subjected; hence, a motion in *parallel* directions, with *unequal* velocities, of a series of unorganized particles, confusedly arranged, must induce a mutual linear distension in a direction inclined to their real motions; and this being unequal for adjacent portions, induces the delicately fibrous part of both, but will undoubtedly rise towards the surface, and thus *slide upwards and forwards* over the particles immediately in advance.

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* The wave-like figures of floating matter on a sluggish surface are not at all to be confounded with the actual direction of motion of the fluid particles. They are curves of *differential velocity* merely, and are always most perceptible near the *sides* of the stream, where the variations of velocity are greatest. A stream, like a mill-race, covered with saw-dust, will shew these linear markings inclining towards the centre of the stream; but the motion of any floating body, as a bit of cork, is sensibly parallel to the sides. I have proved the same thing by

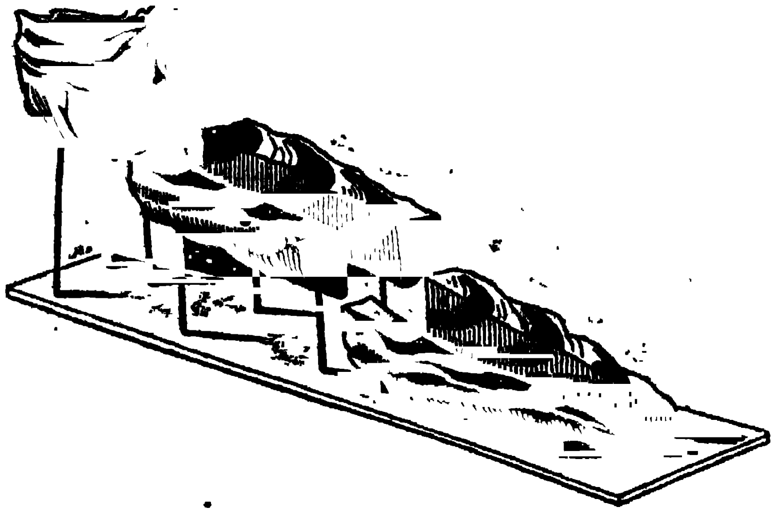
I have succeeded in illustrating this theory by constructing models of a viscid material (a mixture of plaster and glue, which does not *set* readily), poured down irregular channels,

actually performing the experiment suggested in the text, of pouring plaster of Paris and glue into a narrow rectangular box, and sluicing it up by a bit of wood, removable at pleasure. The surface of the viscid mass was then strewed, whilst level, with a coloured powder, and the sluice withdrawn. The liquid flowed exactly as I have described, and the colouring matter was drawn out into threads, precisely resembling, on a minute scale, in delicacy and continuity, the veined appearance of the glacier surface. The explanation appears to be this;—that the velocity of the central strata tends to pull the lateral strata towards the centre, as well as parallel to the length of the glacier; this produces a slight lateral as well as longitudinal discontinuity, for the actual motion of the side strata towards the centre is exceedingly small, and (as the phenomena of moraines tell us, which act like the floating cork in the experiment above described) does not sensibly disturb the parallelism of motion of the parts of the ice. In short, the internal movements are of an order so inferior to the general movement of the stream, that they may probably be left out of account in describing the general movement, although by the fissured structure which they induce, they have sufficient evidence of their existence. But if the slope and consequent hydrostatic pressure be great, the movement towards the centre *may*, as supposed in the text, be of an order to modify appreciably the direction of movement of a particle. In an ordinary liquid like water, the direction of the ripple marks, occasioned by the friction of a stream in proceeding from a wider to a narrower channel, is an example of the same thing, namely, lines of maximum mutual friction of the particles against one another. They converge rapidly towards the centre of the stream, whilst the motion of the fluid, indicated by a floating body, deviates but little from the direction of the axis of the channel.

The same view, *mutatis mutandis*, explains the frontal dip and conchoidal form of the bands between the top and bottom of the glacier. There is here also a *drag* acting from the upper to the lower strata, and fissures are produced in consequence of the sluggish lower strata refusing to follow the swifter upper ones. This may also be a quantity of an order so inferior to the actual rate of motion of the ice, as to make it inappreciable by direct experiment.

The experiment, on a model described in this note, is more strictly analogous to the glacier phenomena, than those of a more striking kind described in the next page, where the succession of colours naturally gives to the mind the impression of a primitive structure near the origin of the glacier, which is mechanically *deformed* into these conchoidal surfaces. They strikingly recall, however, this important fact, that the direction of maximum distension of the particles must be, not parallel to the length of the glacier, but in the direction of the branches of these elongated loops, since their elongation is the simple result of the mechanical tension to which they are subjected; hence, a motion in *parallel* directions, with *unequal* velocities, of a series of unorganized particles, confusedly arranged, must induce a mutual linear distension in a direction inclined to their real motions; and this being unequal for adjacent portions, induces the delicately fibrous representing Alpine valleys. In order to trace the motions better, I composed the streams of alternate doses of white and blue fluid, poured in successively. I have had a great number of such experiments made. The results have been preserved, and sections made of them, which I exhibited to the Royal Society of Edinburgh in March 1843, and described in their Proceedings.* It may safely be stated, that the results of artificial sections of many of these experimental models, were not to be distinguished from the glacier sections transmitted by me from Geneva six months before, as the results of my observations, which are reprinted from the original woodcuts in pages 237, 240, and 241. I subjoin a figure of one of the experimental models of a viscid fluid, which has been sepa-

View of a model shewing the curves generated (experimentally) by the motion of a viscous fluid.



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arrangement of the powder on the surface of the plaster model, and the lamellar arrangement of the ice in the interior of the glacier.

The least distance which can ever exist between a *side* and a *central particle* of a canal-shaped glacier, is half the breadth of the glacier. But the unequal motion of the centre and sides tends continually to separate them wider apart, and to distend the row of particles which connects them. The structural bands are, therefore, perpendicular to the line of greatest tension, and hence crevasses will naturally occur, *crossing the structure at right angles*, which I have found empirically to be the case. In pursuance of this principle the crevasses in an oval glacier are radiating ones; those of a canal-shaped glacier must be slightly convex upwards, and this is perfectly confirmed by the crevassed appearance which the models described at the commencement of this note present. They are fissured in a direction exactly perpendicular to the striæ of the powdered surface.

rated from the bed in which it was run, and divided so as to shew its various sections.

It was objected by M. Agassiz* to this theory of the veins, that were it true, so soon as two glaciers united, they would each lose their individual structure, and have single loops due to the union of their streams, whereas his observations led him to conclude, that the loops of two united glaciers remain distinct. Now, in the first place, I reply, that, though the distinct structure of the double stream is maintained for a time, it is always finally *worn out* if the glacier be long enough, and the structure then forms single loops, *cutting at an angle* the medial moraine of the two glaciers; and, secondly, I

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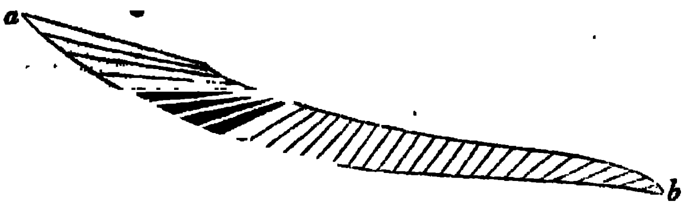
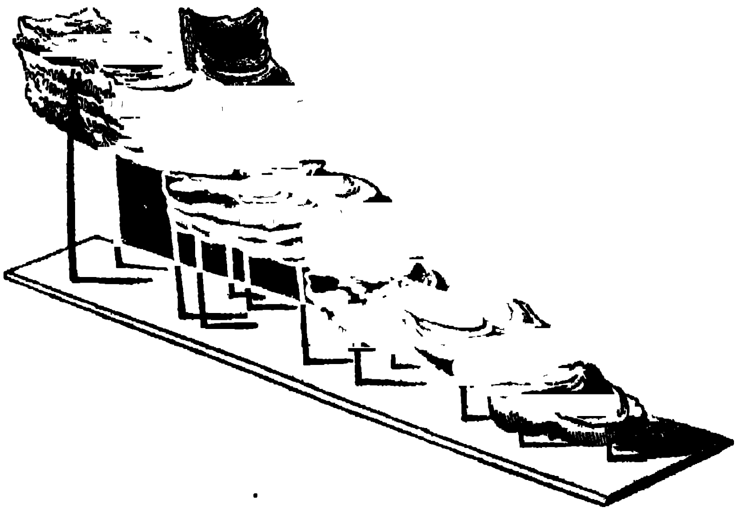
* Proceedings of the Ashmolean Society; *Athenæum Journal*, February 1843. In this communication, M. Agassiz confirms my observation of the "dirt-bands;" adopts the name of "annual rings" (*Edin. New Phil. Journal*, October 1842), and endeavours to prove the conformity of their intervals to the actual motion of the glacier of the Aar, as I had already done on the Mer de Glace. M. Agassiz still insists, that glaciers are *stratified*, and he distinguishes these strata, as he calls the annual rings, from the proper veined structure of the ice. Having maintained, in all his earlier writings, that a glacier is horizontally stratified throughout its whole extent (*Etudes*, page 40), he now adopts my figure at page 237 of this article, for the lower end of his glacier, and connects it with the névé, by a convenient series of interposed strata, first rising, and then falling, as represented in the annexed cut, which is accurately copied from the original in Leonhard and Bronn's Journal, 1843, Heft 1. I can only simply, but distinctly, deny the resemblance to nature of this scheme, and reiterate the observation already several times made in this work, that the structure of a glacier is, and must be formed in the glacier itself, not in the névé, from which it is often separated by an ice-fall, *which has ground the integrant parts of the névé to powder*, as in the Glacier of La Brenva, the Glacier of Miage, the Glacier of Taléfre, and of Allalein, with many others. Not to mention the section of the Glacier of Macugnaga, where the two structures are seen at once, and perpendicular to each other.



Yet more extraordinary is the assumption made by M. Agassiz, in order to account for this supposed prolongation of the beds of the névé into the inferior glacier. In order to explain the alternate rise and fall of the strata, he affirms, that, near the origin of the glacier, the ice in contact with the bed moves faster than at the surface, but everywhere else slower!

maintain that this is precisely what a semifluid body might be expected to do. For the structure near the centre is always imperfectly developed, exactly because *there* the differential motion is least; I mean, that there is least discontinuity of parts, because the velocity is nearly the same throughout a considerable space; and if two glaciers unite, and move tolerably uniformly together, they will preserve, for a long way, the structure which they had already acquired, before the new one (representing a single united stream) is superinduced upon it. Now this is exactly what takes place at the union of the glaciers of Léchaud and Géant,—of the two branches of the Glacier of Taléfre, and of the Glacier of La Noire and Le Géant, all of which, originally double in structure, finally become single, and cut the separating moraine at an angle. But I appealed here also to experiment, and found, that by pouring double streams of viscid plaster down a single channel, the separate forms were *very slowly* worn out indeed, and perpetuated far beyond the point of union of the streams. Thus the proposed objection became a strong confirmation of my theory. One of these models, also shewn to the Royal Society, is represented in the annexed figure.

View of a model, shewing the effect of the union of two streams on the motion of a viscid fluid.



The illustrations now given will, it is hoped, shew that there is a striking conformity between the *facts of motion* and the *facts of structure* in a glacier, and that the two, mutually supporting and confirming one another, lend strong countenance to a theory which includes both. It would be very easy to enlarge upon and multiply these illustrations and coincidences, but I am satisfied that I have said enough to put the intelligent reader in possession of the strong points of the theory, whilst to many this chapter will appear already too long. A few circumstances which have not been insisted on, appeared in the letters to Professor Jameson, published in the Edinburgh New Philosophical Journal, October 1842, and January 1843.

The idea of comparing a glacier to a river is any thing but new, and I would not be supposed to claim that comparison or analogy as an original one. Something very like the conception of fluid motion seems to have been in the minds of several writers, although I was not aware of it at the time that I made my theory. In particular, M. Rendu, whose mechanical views are in many respects more precise than those of his predecessors or contemporaries, speaks of "glaciers d'écoulement" as distinct from "glaciers reservoirs;" and in the quotation at the head of this chapter, he evidently contemplates the *possibility* of the mutual pressures of the parts overcoming the rigidity.* He is the only writer of the glacier school who has insisted upon the plasticity of the ice, shewn by moulding itself to the endlessly varying form and section of its bed, and he is also opposed to his leading contemporaries in his conjecture that the centre of the ice-stream would be found to move fastest. But M. Rendu has the candour not to treat his ingenious speculations as leading to any certain result, not being founded on experiments worthy of confidence. "The fact of the motion exists," he says—"the progression of glaciers is demonstrated; but the manner of it is *entirely unknown.* Perhaps by long observations and well made experiments on ice and snow, we may be able to apprehend it, *but these first elements are still wanting.*"†

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* See also page 107 of his work for a comparison betwteen a glacier and a river.

† "Le fait du mouvement existe, la progression des Glaciers est démontrée; mais le mode est entièrement inconnu. Peut-être avec de longues observations,

I feel bound also to quote the significant expressions of Captain Hall, pointing to the conception of a semifluid glacier. "When successive layers of snow," he says, speaking of the Glacier de Miage, "often several hundreds of feet in thickness, come to be melted by the sun and by the innumerable torrents which are poured upon them from every side, to say nothing of the heavy rains of summer, they form a mass, not liquid indeed, but such as has a tendency to move down the highly inclined faces on which they lie, every part of which is not only well lubricated by running streams resulting from the melting snows on every side, but has been well polished by the friction of ages of antecedent glaciers. Every summer a certain but very slow advance is made by these huge, sluggish, slushy, half-snowy, half-icy accumulations."[*] It is plain, I think, that the author had an idea that liquid pressure might drag a mass over its rocky bed, which would not move upon it as a solid.

But such speculations could not pass into a theory, until supported by the definite facts of which M. Rendu deplores the want. I too, like my predecessors, though independently of them, had compared the movement of glaciers to that of a ductile plastic mass, in 1841, when I spoke of the Glacier of the Rhone as "spreading itself out much as a pailful of thickish mortar would do in like circumstances,"[†] and again, when I likened the motion of glaciers to that of a great river, or of a lava stream.[‡] But I knew very well that such analogies had no claim to found a *theory*. I knew that the *onus* of the proof lay with the theorist,—(1.) To shew that (contrary to the then received opinion) the centre of a glacier moves fastest;

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des expériences bien faites sur la glace et la neige viendra-t-on à bout de le saisir; mais ces premiers éléments nous manquent encore."—*Théorie des Glaciers*, p. 90.

[*] "*Patchwork*," vol. i., p 104, *et seq.* The whole passage, which is too long to quote, gives an admirable picture of the glacier world.

[†] *Ed. New Phil. Journal*, January 1842.

[‡] *Edinburgh Review*, April 1842, p. 54. Both these articles were written in 1841.

and, (2.) to prove from direct experiment that the matter of a glacier is plastic on a great scale, a fact which seems so repugnant to first impressions as lately to have been urged in a most respectable quarter,* as rendering the doctrine of semifluid motion untenable. No one had a right to maintain the theory of fluid motion as more than a conjecture, until at least these preliminary obstacles were removed by direct observations.

These observations have been made, and the result is the viscous or plastic theory of glaciers, as depending essentially on the three following classes of facts, all of which were ascertained for the first time by observations in 1842, of which the proofs are contained in this work.

1. That the different portions of any transverse section of a glacier move with varying velocities, and fastest in the centre.

2. That those circumstances which increase the *fluidity* of a glacier,—namely, heat and wet,—invariably accelerate its motion.

3. That the structural surfaces occasioned by fissures which have traversed the interior of the ice, are also the surfaces of *maximum tension* in a semisolid or plastic mass, lying in an inclined channel.

There is only one other point to which I would invite attention, and it is this. We have noticed the enormous depression which the surface of the ice undergoes during the warmer months of the year. We may be sure that, in some manner or other, this is made up for during winter and spring. I already suggested, in my fourth letter to Professor Jameson (Edin. New Phil. Journal, vol. xxxiv. p. 1), that this may be partly owing to the dilatation of the ice during winter by the congelation of the water in its fissures, producing, at the same time, "the veined structure." The glacier is very far indeed from being frozen to the bottom in winter, for we have seen that physical principles are opposed to this, as well as the fact that, the motion continues during all that period, shewing that a great portion of the icy mass is still plastic. It is, however, extremely probable that the congelation extends to a considerable depth, and produces the usual effects of expansion. I think, however, that the explanation, though correct so far as it goes, is inadequate, and that the main cause of the restoration of the surface is the diminished fluidity of the glacier in cold weather, which retards (as we know) the motion of all its parts, but especially of those parts which move most rapidly in summer. The disproportion of velocity throughout the length and breadth of the glacier is therefore less, the ice more pressed together, and less drawn asunder; the crevasses are consolidated, while the increased friction and viscosity causes the whole to swell, and especially the inferior parts, which are the most wasted. Such a hydrostatic pressure, likewise, tending to press the lower layers of ice upwards to the surface, may not be without its influence upon the (so-called) rejection of blocks and sand by the ice, and may even have some connection with the recurrence of the "dirt bands" upon the surface of the glacier. But I forbear to enlarge upon what is only as yet to myself conjectural.

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* *Bibliothèque Universelle*, January 1843.

I have no doubt, however, that the convex surface of the glacier (which resembles that of mercury in a barometer tube,) is due to this hydrostatic pressure acting upwards with most energy near the centre. It is the "renflement" of Rendu, the "surface bombée" of Agassiz. Exactly the contrary is the case in a river, where the centre is always lowest; but that is on account of the extreme fluidity, so that the matter runs off faster than it can be supplied; but in my plaster models, this convexity, with its wrinkles and waves, was perfectly imitated.

In its bearing on the theory of the former extension of the Swiss glaciers, we find, that the doctrine of semifluid motion leads us to this important conclusion,—that as large and deep rivers flow along a far smaller inclination than small and shallow ones (a circumstance depending mainly upon the weight increasing with the *section*, and the friction, in this particular case, with the *line of contact* with the channel), the most certain analogy leads us to the same conclusion in the case of glaciers. We cannot, therefore, admit it to be any sufficient argument* against the extension of ancient glaciers to the Jura, for example, that they must have moved with a superficial slope of one degree, or, in some parts, even of a half or a quarter of that amount, whilst in existing glaciers the slope is seldom or never under 3°. The declivity requisite to insure a given velocity, bears a simple proportion to the *dimensions* of a stream. A stream of twice the length, breadth, and depth of another, will flow on a declivity half as great, and one of ten times the dimensions upon 1-10th of the slope.†

Poets and philosophers have delighted to compare the course of human life to that of a river; perhaps a still apter simile might be found in the history of a glacier. Heaven-descended in its origin, it yet takes its mould and conformation from the hidden womb of the mountains which brought it forth. At first soft and ductile, it acquires a character and firmness of its own, as an inevitable destiny urges it on its onward career. Jostled and constrained by the crosses and inequalities of its proscribed path, hedged in by impassable barriers which fix limits to its movements, it yields groaning to its fate, and still travels forward seamed with the scars of many a conflict with opposing obstacles. All this while, although wasting, it is renewed by an unseen power,—it evaporates, but is not consumed. On its surface it bears the spoils which, during the progress of existence, it has made its own;—often weighty burdens devoid of beauty or value,—at times precious masses, sparkling with gems or with ore. Having at length attained its greatest width and extension, commanding admiration by its beauty and power, waste predominates over supply; the vital springs begin to fail; it stoops into an attitude of decrepitude;—it drops the burdens, one by one, which it had borne so proudly aloft,—its dissolution is inevitable. But as it is resolved into its elements, it takes all at once, a new and livelier, and disembarrassed form;—from the wreck of its members it arises, "another, yet the same,"—a noble, full-bodied, arrowy stream, which leaps, rejoicing over the obstacles which before had staid its progress, and hastens through fertile valleys towards a freer existence, and a final union in the ocean with the boundless and the infinite.

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* Elie de Beaumont, *Annales des Sciences Géologiques par Rivière*, 1842.

† This results approximately from the formulæ of Dubuat and Eytelwein,—the velocity varies as the square root of the slope, and as the quare root of the mean hydraulic depth.

Abstract of a Paper on Wood-Paving. By D. T. HOPE, Esq., F.R.S.S.A., Civil Engineer, Liverpool. Communicated by the Royal Scottish Society of Arts.*

The excellency of wood, as a material for paving, is now so generally admitted, that it may seem unnecessary to inquire into the advantages it possesses over stone-blocks or Macadamization.

And assuming the superiority of wood as sufficiently proved, by the rapid progress it has made in the public estimation, and the very favourable results of its varied applications in the most bustling thoroughfares in London, and some provincial towns in England,—the subject of inquiry may be usefully directed to the best position of the fibre of the wood,—its durability and efficiency as a material for paving, under wet, dry, and frosty weather; and into the value of animal power in draught on wood-pavement.

The patentees of wood-paving are divided into two classes: one party for the fibre in a vertical position,—and the other party for the fibre at *a particular* angle.

The general utility of the subject induced me to pay particular attention to it for some years back; and with the view of ascertaining the respective merits of the several descriptions of pavement and roadways, I made a variety of experiments on Macadamized roads and stone and wood pavements; on wood with the fibre placed vertically, and at angles from vertical to horizontal.

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* Read before the Royal Scottish Society of Arts, 27th March 1841.

In the experiments on wood paving (the results of which I now submit), the blocks were laid in the best manner, on the same sort of concrete substratum, and on the same line of road; so that all varieties had the same amount of traffic, and the same attention paid to their almost daily examination, for the space of eighteen months.

I. *On the Position of the Fibre.*

It will be quite unnecessary to notice the arguments that have been adduced to support the several systems in practical operation, being, under circumstances, such as locality and traffic, which prevent comparisons being accurately drawn. These I have, however, guarded against, and the measurements taken were not of single blocks but of many.

I may here remark, that I am decidedly of opinion that a superior concrete substratum is absolutely necessary, and an essential feature in the successful application of wood for paving.

A reference to the following table will shew the exact amount of wear which wood blocks, with the fibres varying by 15 degrees from vertical to horizontal, sustained for the first, second, and third months, and each successive three months to eighteen months; and also of granite sets for the same period.

Amount of Wear Sustained.	Fibre Vertical.	Fibre at 75 deg.	Fibre at 60 deg.	Fibre at 45 deg.	Fibre at 30 deg.	Fibre at 15 deg.	Fibre Horizontal.	Granite Sets.
At the end of								
1 Month......	.017	.023	.032	.046	.065	.088	.109	.014
2	.030	.038	.051	.069	.093	.120	.154	.025
3	.040	.051	.065	.088	.114	.149	.189	.037
6	.062	.078	.101	.136	.178	.231	.294	.073
9	.078	.095	.120	.167	.220	.278	.363	.112
12	.096	.115	.142	.194	.253	.312	.390	.141
15	.111	.132	.164	.219	.282	.347	.433	.183
18	.125	.147	.182	.241	.312	.379	.480	.218
Proportions of an inch for the 18 months.........	$\frac{1}{8}$	bare $\frac{6}{40}$	bare $\frac{3}{16}$	bare $\frac{1}{4}$	$\frac{5}{16}$	full $\frac{3}{8}$	bare $\frac{1}{2}$	$\frac{7}{32}$

It appears that the amount of wear is greatest in the first month, and gradually decreases every subsequent month. For instance, the wear for the first and second months is greater than any three months from the sixth. This, I think, can be very satisfactorily accounted for. Although the depth of the blocks diminished more in proportion for the first and second months, yet they did not seem to have lost much by abrasion. They had undergone compression, and presented a more compact surface than when laid down; and, besides, being more compact in fibre, the surface was so impregnated with fine sand, that it had more the appearance of stone than wood.

1. The vertical fibre blocks during the eighteen months were only diminished in depth, between compression and abrasion, .125, or $\frac{1}{8}$th of an inch. The blocks at the end of that time were in as good condition as if they had not been exposed to heavy weights and abrasion.

2. The blocks with the fibres leaning at an angle of 75 degrees, shewed the additional wear of .022 at the end of the eighteen months, which is the 40th part of an inch more than if they had been vertical. The surface shewed a greater abrasion of the soft fibres, and the resinous fibres were slightly pressed to the leaning side.

3. The next are those with the fibres at 60 degrees. At the end of the first month, the diminution in depth was .032, nearly double that of vertical; and at the end of the eighteen months they were diminished .182, which is about $\frac{3}{16}$ths of an inch; clearly shewing that blocks at that angle must lose $\frac{1}{16}$th of an inch more than vertical blocks. The surface was not so regular as the preceding, occasioned by the larger circles of soft fibre sustaining greater abrasion, and these as well as the smaller circles being unable to resist so much pressure at that angle, were so squeezed as to lose their cohesion on the immediate surface; and the resinous fibres being also unable to resist pressure at that angle, instead of protecting the softer, were leaning on them, and to a small extent shewed a tendency to separate into threads.

4. The blocks with the fibres leaning at 45 degrees, lost rather more at the end of one month than the vertical did at

the end of three months; and at the end of the eighteen months about double what the vertical sustained. The surface was very much like the preceding, but the soft fibres had suffered more abrasion, and the resinous were separating into threads nearly $\frac{1}{8}$th of an inch.

5. The blocks with the fibres at 30 degrees lost more in one month than the vertical did in six; and at the end of the eighteen months $\frac{3}{16}$ths more than the vertical. The surface was similar to the last, but to a greater extent.

6. The block with the fibres leaning at 15 degrees, lost as much in one month as the vertical did in ten; and in eighteen months full $\frac{3}{8}$ths of an inch, being three times more than the vertical. In proportion to the angle, the surface was getting more unequal, suffering greater abrasion, the threads becoming longer and irregular, and the general appearance shewing that destruction was making rapid progress.

7. The last to be noticed are blocks with the fibres horizontal. For the first month the wear was equal to fifteen months of the vertical, and in eighteen months they lost about half an inch. The fibres were completely separated to a considerable depth, and the surface had the appearance of a heap of broken strings.

II. *On the Durability of Wood as a Material for Paving.*

It seems indeed strange, that such incompressible and durable substances as basalt and granite should be more subject to wear, with the same amount of traffic, than wood with the fibre presented to the pressure and percussion. The former, however, when acted on by the wheels and horses' shoes, resist the pressure and percussion, and thereby have their particles abraded into a very minute sand; and the iron is also subjected to a diminution in proportion to the hardness of the stone.

On the other hand, wood, from its elasticity, yields to the pressure, and permits the weight to pass over it without any sensible injury to either the iron or wood.

A reference to the table will, however, shew the amount of wear wood and granite sustained with the same traffic. The first column will shew that wood lost 1-8th of an inch, but more from compression than abrasion; and the last column will shew that granite sets lost 7-32ds of an inch from abrasion alone,—which proves that the elasticity of wood is that which renders it durable and applicable for paving; and that the non-elastic property of stone is the cause of its inferior durability.

III. *On the Efficiency of Wood for Paving, when necessarily subjected to Wet and Dry Weather.*

It is now sufficiently established, by men of maritime experience and ship-builders, that those parts of a vessel which are constantly exposed to the water, are never found to be the least affected, while other parts of the same vessel are undergoing rapid decay; and that decay is even arrested when it reaches the same seasoned parts.

Wood blocks in pavement may thus be said to be quite exempt from the probability of decay, even although they should be perfectly dry when laid down (a condition to be particularly recommended). They are placed on a humid, or what will soon become humid, substratum, closely packed to each other, and totally excluded from atmospheric influence, saving the surface. In wet weather they absorb as much moisture as they can contain, which renders them more adhesive and compact; and from which moisture they are never after totally free, even in the dryest weather; for wood being a bad conductor of caloric, any variation of the atmosphere has little effect on the blocks, or the surface exposed to it.

To prove this, I weighed a number of the blocks when laid down; and, after having been in use till properly moistened, had them taken up and reweighed, when I found that they had gained by moisture 4¾th ounces. After a long continuance of dry weather, I had them again taken up and weighed, and found that they were still moist, having lost only 1¾th ounce. I also split some of the blocks, and found that they were moist to the core, except about an inch from the surface, but regained that moisture towards the evening. I tried this experiment frequently, with similar results. The small difference in the size of the blocks, under various degrees of temperature, also bears out these experiments. The medium variation I could discover in their volume was .057, which I attributed to the loss or gain of moisture; and this trifling difference of volume did not affect the adhesion of the blocks, or the general structure, in consequence of the moisture they retained in dry weather still maintaining an excess of volume over the dry state in which they were originally laid down.

To discover if it was from the exclusion of atmospheric influence that the moisture was retained, and occasioned their limited expansion and contraction, and not any peculiarity of the wood of the precise blocks used, I found that, in wet weather, blocks, when taken out and kept singly, expanded as freely as those used for comparison; and when they became the exact weight they were when taken out in dry weather, their volume was proportionally greater; and when kept till perfectly dry, they were reduced to their original dimensions.

I also found that the moisture contributed much towards imparting additional strength to the fibres of the wood, and rendering it more capable of resisting pressure and abrasion, besides preserving it from dry rot.

IV. Having glanced at the durability of wood under wet and dry weather, the next thing to be considered is the effect which frost has upon it.

In Russia, where the climate is so rigorous, frost might be supposed to be an insuperable objection; but when we find that it was in that country, some centuries ago, where wood paving was first adopted, and that a system not inferior to what is practised here, has been there for many years in general use, we may consider that, in the mild climate of Britain, where frost is neither severe nor of long duration, it can only be of trifling consequence.

In Russia it is admitted that frost has a deleterious effect on the wood,—to counteract which, they give it an annual coating of tar covered with sand, which, with other advantages, obviates any excess of slipperyness.

I found that the surface coated with common varnish and sand was highly beneficial. It preserved a more uniform temperature (although, as I have stated, the want of this greater uniformity is not objectionable), and rendered the surface rough and less slippery in frosty weather.

Laying aside this additional surety, in the course of two winters, I could find no other objection to frost than the slipperyness; but which, from the sand introduced between the fibres, and the grooving, was really no worse than on any other pavement; and even less than on a smooth macadamized road. And, in regard to the effect on the timber, I could discover nothing that was injurious; for the frost did not penetrate deep, and I did not find that the substratum was frozen at all.

V. *On Traction on Wood Pavement.*

Wood is eminently superior to any other material which has yet been employed for enhancing the value of animal power in draught, from its elasticity, and its peculiarity of maintaining, in all seasons and conditions of the weather, the same compact and even surface.

Besides the absence of surface resistance on wood, the power of the horse is materially increased when acting on the elastic surface. The resistance which the foot of the animal meets with on stone pavement is communicated throughout its whole body, reducing its power of action at the time, as well as the duration of its working life. But in wood pavement this resistance is partly borne by its superior elasticity, which receives a portion of the shock, and diminishes the injurious effects of percussion on the hoof. The muscular energy of the animal is in proportion saved—the abrasion of the pavement is reduced—and the wear and tear of carts and vehicles is diminished.

To ascertain the weight which a horse could draw, with the same exertion, and at the same rate of speed, on a macadamized road, on granite, and on wood pavements, I found the following to be the proportions deduced from a variety of experiments:—

	Cwts.
On granite pavement, ...	28
On a macadamized road, ...	$34\frac{5}{7}$*
On wood pavement, ...	50

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* The weight on the macadamized road cannot be considered as a constant

From the foregoing experiments it may be inferred,—

That verticality of fibre is the most durable position of wood for paving, besides affording the means of obtaining as firm a structure as is requisite.

That wood is an efficient material for paving, whether subjected to wet, dry, or frosty weather.

That the moisture it constantly retains increases its strength, preserves it against dry rot, and undue expansion and contraction.

That wood for pavement is more durable than granite.

That the value of the horse is materially enhanced, and its power in draught considerably increased on wood pavement. And,

That, with its general adoption, steam power may be successfully employed.

D. Hope.

26th April 1843.

Observations on some of the Decorative Arts in Germany and France, and on the causes of the superiority of these, as contrasted with the same Arts in Great Britain. With suggestions for the improvement of Decorative Art. By Charles H. Wilson, Esq., A.R.S.A., V.P.R.S.S.A. Communicated by the Royal Scottish Society of Arts.*

The general promotion of taste is an object of great interest and importance, and materially affects our commercial prosperity. We find it asserted on authority which cannot be questioned, that the principles of taste, as applied to manufactures,

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quantity, the quality of the surface being so subject to variations; for instance, the same power will draw on a macadamized road,

	Cwts.
Smooth and consolidated, ...	34¼
After a shower of rain, ...	20¼
During a continuance of wet weather, . .	23¾
Laid with new metal, ...	10

* Read before the Royal Scottish Society of Arts 24th April 1843.

are better understood on the Continent than with us; and the subject has been deemed of so much importance that parliamentary inquiries have been made into the causes of our inferiority, which inquiries have been followed by active exertions on the part of Government to promote improvement of taste amongst the manufacturing classes, by the establishment of Schools of Design in London and elsewhere, also by the passing of a Copyright Bill, by which an effort has been made to protect the authors and proprietors of novel designs from the piracy which has been so injuriously practised.

There can be no doubt that these measures are important steps, and must tend to promote the objects which those who originated them had in view; but we must not rest here,—we must do much more than has yet been done, or perhaps ever contemplated, before we can hope to meet our neighbours without disadvantage in the display of taste. We must not only, as they do, teach principles of good taste in Schools of Design, and defend honest men from the piracy of knaves, but we must also, as they do, form and throw open to our people extensive museums of art, employ the painter and the sculptor to complete the edifices which are raised by the skill of our architects, call in the aid of the fine arts in commemorating the glories of our country, and unite the labours of the artist with those of the historian.

We must, I think, attribute the superior taste which our neighbours exhibit in their manufactures and decorative arts* in a great measure to the advancement which they have made in the fine arts. I have been unable, in speaking of the former, to omit allusion to the latter, and I do not wish to separate them. The divorce which in our day and amongst us has taken place between fine art and ornamental art, has been in many instances fatal to the latter, and certainly has been of no advantage to the former. It has indeed been asserted that taste in manufactures has nothing whatever to do with the state of the fine arts, but the whole history of art proves the reverse of this proposition; and while there can be no reasonable doubt that many of those decorative arts which are more immediately connected with fine art, rise and fall with it; so do I not doubt that every manufacture where taste can be shewn, however apparently unconnected with fine art, is influenced by its actual state. It will hardly, I think, be denied that on taste in architecture depends that in house-painting, in furniture, and in iron-work. In every age the forms and ornaments which have been used in these arts have been in accordance with the architectural taste of the time, and, in fact, we can at a glance tell to what period an old piece of iron-work or carving belongs. I have said that a divorce has taken place between fine and ornamental art, and in the medley designs of our house-painters, cabinetmakers, and smiths, the unhappy effects are sufficiently discernible. Assuredly, some centuries hence, should the works of our artizans survive to such a period, they will puzzle the artist and antiquary of these days to decide to what age they belong.

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* By decorative and ornamental art, I mean that art which is not usually classed by us with fine art. Neither the expressions nor the distinction are correct; but, as I must make a distinction, I use these phrases for want of more appropriate ones in our vocabulary.

The majority of modern architects leave their works to be completed by the house-painter, and, I may add, the upholsterer and smith, for a house cannot be said to be complete till these artizans have worked in it, and for it.

If the house be Greek or Italian in architectural style, most house-painters on being consulted will recommend its being painted "à la Louis XIV.;" if Elizabethan, they will still warmly advise the Louis XIV.; and if Gothic, they will advocate the same style. The upholsterer thinks of no style at all, neither does the smith. I am not, indeed, aware, that either are ever called upon to furnish a house in accordance with its architecture; such a thing is never, or, at any rate, rarely, and then only very partially, thought of. We shall see in the course of my observations on foreign buildings, whether such is the case in these.

That a house should be painted in any style but that of its architecture is preposterous; we acknowledge this at times in the completion of public or religious buildings. Why should it not be the universal rule; for certainly furniture might be made as comfortable as modern habits require, and still be in harmony in point of form and taste with the edifice: what a charming variety would thus be attained!

It is not my intention to dwell at any length upon the defects of our system; these, indeed, would themselves require more than one paper. It must, however, be said, in justice to our citizens, that what they have done is wonderful. When we consider the actual neglect with which their interests have been treated, their merit is very great; indeed, they have done as much as it is possible for mere artizans to do, and have carried some of the ornamental arts to as high a perfection as can be attained by men who have not the education of artists, and who are not instructed by their example.

I repeat that I cannot omit all mention of fine art, although the present paper bears a title which might authorize the supposition that I intend to confine myself exclusively to descriptions of arts which, according to our present mode of thinking, have no connection, or, at any rate, a very slight and distant one with the *fine arts*. As I mean to endeavour to shew that the contrary is the case, I shall briefly touch upon the state of our School of Art, and offer a few observations upon the state of taste in this country. I shall then glance at the state of art in Germany and France, and contrast the system adopted in those countries for the encouragement and employment of art with that followed in ours.

The history of the fine arts in Scotland is very remarkable. We have seen a school struggle into very considerable excellence under circumstances of discouragement and neglect unparalleled in the history of art. Our public is very ready to congratulate us on this advancement; but the chief merit is the artists', and the public is very far from being entitled to any great share of it. Art could not exist at all without some encouragement; but if so much has been done by the artists with so little aid, what might have been done had there been a more general appreciation of the importance of the fine arts. The public, generally speaking, are by no means so far advanced in point of taste, as the state of art in this country should have made them. We may easily form a judgment on this subject by our intercourse with society, in which even a moderate knowledge of art will be found to be confined to a very small minority. Then, if we turn to our press for proofs of knowledge, whilst with one or two exceptions in London the lucubrations in our papers are too frequently beneath contempt, our more important journals almost entirely neglect the subject; and this neglect is a sufficient proof how little it is in reality valued, or even thought about.

But I hope that we may now look forward to better times; every where there are indications of improvement. There seems to be an increasing desire on the part of the public to see art encouraged. Institutions have been established, and societies formed, with this avowed object, and so far it is pleasant to contemplate this; but these very efforts, although made in an admirable spirit, are misleading us. We congratulate ourselves on our exertions, and believe that we are exhibiting an earnestness in the promotion of the fine arts worthy of our place amongst civilized nations; but I greatly fear that, from the very nature of the means which we are adopting, and from the want of just ideas on the subject, whilst some good is effected, we are also ministering to all the evils which afflict our school,—we are fostering and perpetuating a system which would be thought the invention of insanity, if followed out by any student for any other profession, but which is apparently thought the most suitable preparation for becoming an artist.

Have we made a single effort of importance to establish a proper school for the due education of artists? The fact that we have not, is a sufficient proof that we have not thought it worth while. It is true that in this town a magnificent establishment exists, but it was founded with different views; and, besides, it will hardly be maintained by any reasonable being, that a few hours' teaching in the week, and permission for students to draw during the day, without any guidance whatever, is sufficient; would such a system make either lawyers or physicians? The artist alone is left to grope his way almost unaided. I am, indeed, no friend to any mere academic system for the education of artists, but such would be much better than none at all; and as we can hardly hope to see the old system of schools restored, I should be glad to see an academic school amongst us offering certain advantages to the student, whilst the disadvantages of the system might in some respects be avoided.

The artist in this country has thus two great evils to contend with; in youth, the want of the means of education—in manhood, the want of proper and well-directed patronage. Private patronage can create a very excellent school of art, but it cannot create what we term a great school. We must have that of the State, and also that of Municipalities; I would willingly add also that of the Church, but that is very hopeless in Scotland. I am certain, however, that if the State finally patronizes art (and I am thankful to think that there is now a certain prospect of its doing so), our municipalities will follow and do so also. We should then see our artists called upon to design, not only great historical works, but also works which would bear more directly on mere ornamental art, than the production of pictures alone can possibly do, and which would, therefore, tend to its improvement; and, as I hope to shew you by my subsequent observations on foreign art, whilst the artist would profit in every point of view by such employment (his field of study, for instance, would be greatly extended, which would unquestionably tend to the general improvement of fine art), the position of the ornamentalist would also be greatly improved, more ability and cultivation would be required in his department; and as juster ideas of art would soon prevail, young men would not so readily esteem themselves fit to be artists, as they do now on very slender grounds, but would continue in departments of art which would offer them secure subsistence, rather than embrace the miserable and hopeless career of the mediocre artist.

In my late continental tour, my express object was inquiry into certain processes of painting; but although much occupied with these, I still had time to give a passing glance to other interesting subjects so closely connected with the particular objects of my journey, that I had merely somewhat to extend my observations partially to embrace these also.

The King of Bavaria is the greatest patron of art now living, and in his capital we may see numerous proofs of the results which a well-directed patronage of the arts can produce. The Bavarian artists now enjoy an European reputation, but it is much to be regretted that the zealous praise of some of their admirers amongst ourselves has raised a feeling in some of our artists, which displays itself in *discreditable* abuse in those journals, the pages of which are particularly devoted to art; thus we have on one side an admiration which, although just, is too exclusive, and, on the other, criticism, which is intemperate and ungenerous.

No comparison whatever can at present be instituted between the leading artists of Germany and those of this country: when our artists are, like the former, employed to paint national monuments, then we may institute a comparison, but, at present, none can with justice be entered upon. To paint a single historical work, however large it may be, is one thing, but to paint a series for a particular building, is quite another; it is possible that an artist may succeed in the single picture, yet fail in the series. Where a comparison can fairly be instituted, and that is between the cabinet pictures, landscapes, and portraits of our School and of theirs, I think that it cannot be doubted, that, in many respects, our artists have the advantage, and we may entertain a warm expectation of success when they are called upon to execute works of equal magnitude and importance with those of their continental brethren.

The King of Bavaria has resolved that his capital and dominions shall contain monuments to rival those erected by the magnificence or piety of former days, and he has to a wonderful extent succeeded in his object.

The manner in which His Majesty meets his artists is interesting, and offers, I think, a useful lesson to our amateurs. When he has resolved on the erection of a new church or other important edifice, he summons an architect, painter, and sculptor to his presence, and explains his wishes to them either separately or together; when the plans are ready, the artists again meet their sovereign, and a council is held over them; he encourages them freely to express their opinions, even when contrary to those expressed by himself. When every thing is thus at last decided upon, the work is commenced and goes on without interference; and should any part of it prove less successful than was expected, there are no reproaches, for the King at the previous council took his share of responsibility.

It will be easy to conceive that such a monarch is spoken of with devoted attachment by the men he thus employs, and that he is served with enthusiasm. Many express surprise that the King of Bavaria should have been able to carry on and complete such varied and extensive works, when it is known that the resources of his kingdom cannot be very great; but he is aided by the devotion of his artists, who accept of moderate sums for their labour; many of them will leave immortal names, but few of them indeed will leave fortunes.

I beg that it may not be thought by these expressions that I am of opinion that art should ever be poorly paid. I hold a very opposite opinion; it ought to be well paid, but not extravagantly, as some seem to think. I regret to think that, in our country, public undertakings are so frequently viewed by individuals, employed in them as sources of immense emolument; hence an outcry, hence opposition to every undertaking that is not of the most utilitarian character, and the apprehension of extravagant cost frequently deters from many undertakings that would be beneficial to art.

I admire the Bavarian artist who is content with the emolument which his king can afford to give him, and who undertakes works at a moderate price for the love of art and the honour of his country; and this spirit prevails amongst all who are employed, amongst artizans, as well as artists. The Chevalier Klenze, the king's principal architect, informed me that the operatives bestowed so much time and labour upon every thing that they undertook for the king, that in the earnest desire to make their work as perfect as possible, they seriously impaired their profits. These are interesting facts, and assist, at any rate, in explaining how so many works are done, and so well done.

The tendency of the Germans in art has been much misrepresented in this country, and we have heard it repeated, *usque ad nauseam,*" that they are mere imitators of the very early masters. This is not true. I shall not enter at large upon this subject, but beg to refer you, for what I believe to be a true view of it, to Mr Eastlake's admirable paper at the end of the last report of the last Parliamentary Committee on the Arts.

In architecture, I am not disposed to consider our friends so favourably;—there is much genius evinced in their productions, their conceptions are great, and magnificent works are undertaken, and brought to a successful termination, but their talent is chiefly shewn in very direct imitation, and that imitation is not always discriminating; there are many very tasteful revivals of the middle age Tuscan, of the restored Italian classic, of the Byzantine and Romanesque, but, at the same time, there is also a revival of the principal defects of the Italian architects; and I do not think that much judgment is always shewn in the choice of a style. The famous Ludwig Strauss is wholly ineffective as a street; the style of most of the buildings is that of the fortress palaces of Tuscany, and the imitation is not at all times successful. The material, however, is excellent, and so is the workmanship; the details are generally in beautiful taste and admirably executed, and the decorative completion of the buildings is ever in a style of great magnificence.

The few attempts in Gothic are coarse, and almost entirely devoid of all true Gothic feeling; and it is remarkable that the details which, in edifices in other styles, are better than the general designs, are, in the Gothic attempts, very indifferent and inferior to the conception of the mass.

I do not think that the Bavarian School of Sculpture has any very high claims to excellence. The word clever seems to me the most applicable to the works which I saw at Munich. There is no want of employment however. In the new throne-room of the palace, there are twelve colossal portraits of ancestors of the king, in gilt bronze; the Tympana of the Walhalla, the Glyptothek, and portico opposite, are filled with statuary; and I might mention much besides; but the most extraordinary undertaking of all, is a statue of Bavaria now modelling, and which is to be cast in bronze.

It seems to be about fifty feet high; and I saw several young sculptors perched on different parts of it, or slung with ropes, chipping away at the plaster of which it is formed, and shaping it with small pickaxes as substitutes for the usual modelling tools.

Whilst the fine arts in Munich are flourishing, the decorative arts which are connected more immediately with them are in a very advanced state. The house-painters of Munich are excellent artists, and paint cleverly in oil, fresco, encaustic, fresco secco, a peculiar art imported from Italy, and in common distemper. The reason of this ability and advancement is evident. The arabesques and ornamental painting in the palace are all designed and executed by eminent artists, and so it was in the best ages of art. Thus, an example of fine designs, correct and appropriate taste, and excellent execution, is set to the mere decorative painter, many of whom, indeed, are employed as assistants, and thus study their art under the most advantageous circumstances. The reason of our inferiority in this department is thus rendered evident, and all efforts to place ourselves on the same level with these artists will be vain, till we see the same system adopted.

The execution of ornamental architectural details at Munich is also excellent; there is no art in which we are more deficient than in this, as is sufficiently evinced in the hard, stiff, and lifeless character of our architectural ornamental details of every description.

I was also much struck with what may be termed the decorative carpentry, or rather joinery, at Munich. I am enabled to shew you some examples of the beautiful flooring of the palace; the cost is, however, considerable, about 3s. 4d. for every 18 inches square, which would make a cost of L.100 for a room 30 feet square, although machinery is used in the formation of the pieces of which this mosaic work is formed. The doors also of the palace are beautiful specimens of taste and workmanship; they are about 10 feet high, and formed of various fine woods inlaid in beautiful patterns and highly polished; each door costs L.18. I have not seen any thing to equal them in any other royal residence which I have visited.

Metals, also, are wrought with great taste and skill; and in ornamental work, attention is paid in the design to the *nature* of the material, which is too much neglected by our designers, amongst whom forms borrowed from those of stone-work are generally used in iron.

I purchased, for the use of our School of Design here, a number of examples of ancient iron-work, made in the workshops of Nuremberg, and which unquestionably excel both in taste and in workmanship the boasted productions of our day. It is perhaps impossible to restore, for all purposes, the old modes of working iron; but although we must submit to the trammels of casting processes, yet in designing even for these, just principles of design may be introduced, by paying more attention to the nature and capabilities of the material.

I now beg to call your attention to another important art which has been restored and is practised with much success in Munich; I mean that of glass-painting. Before entering upon a description of it, I would beg such of you as have seen them, to recall to your memories the noble specimens we possess in some of the cathedrals and ancient churches in the south; I would mention the fine windows of Cologne Cathedral, but especially those of St Lawrence in Nuremberg, in which church the Volkamer window may be mentioned as, in all probability, the finest in the world. The art has never been lost in Nuremberg, and I am happy to shew you a copy, by the best artist of that place, of a portion of the Volkamer window. You observe that we have here a figure of St Catherine, admirably drawn, and she is placed over a Gothic pattern or ornamental design, which runs through the greater portion of the window behind the figures. You have here a specimen of the true system on which such subjects on glass should be designed. These should be treated in a conventional manner; no attempt should be made to represent nature, as we do, for instance, in a picture, as thereby the idea of a window is immediately destroyed; many of you who have seen it must have been struck with the bad effect produced by this mode of painting a window, as seen in St George's Chapel at Windsor, in St John's Chapel here, and in the Parliament House. Notwithstanding the just criticism with which these have been assailed, glass-painters, both in the south and amongst ourselves, persist in copying pictures for such purposes, so little do they understand the principles of design, as applicable to their art. I saw in London a copy from Ruben's descent from the cross, being executed for a church, and I might cite many other examples of this perversion of taste.

Now, the glass should be painted with architectural ornaments in character with the architecture of the church, and these should be correctly coloured in imitation of ancient painted examples of church architecture. Some of you are aware that both the exteriors and interiors of ancient buildings were richly painted. It was thus in Egypt, thus in Greece, and such was the practice in ancient and Gothic times. It was a practice which, I believe, was abandoned when the principles of taste were better understood, although I say this with caution, and it would be foreign to my subject to enter upon this interesting question. The architectural and ornamental design, then, in church windows, in the particular examples which I bring before you, seems to be a representation, in brilliant colours, of the painted architecture of the period, and over these are painted the figures, whether of holy personages, saints, or heroes.

The architectural ornaments or design fill the whole window, and the figures are drawn and painted in a severe manner, without any affectation of pictorial effect as to light and shadow.

To give you a more distinct idea of my meaning, besides these specimens of painted glass, I exhibit a coloured engraving from one of the windows of the Au Kirche at Munich; in this specimen the true principles of design, as I view them, have been adhered to with considerable fidelity, although such is not exactly the case with all the windows in that church.

I have made these brief observations upon this important subject, because, as far as I can judge from the examples which I have seen, neither in London or any where in this country, is the art of designing for glass-painting yet understood. The windows which you frequently see executed of pieces of stained glass arranged in patterns, cannot be criticized as specimens of the art at all; those in which ornaments are painted are very far from satisfactory, and as to the copies from ancient masters, from Mr Martin's coloured prints, the portraits of noble lords, &c. &c., the sooner these are sent to the glass-house, to be melted for some useful purpose, the better.

There is a school of glass-painting at Munich, fostered by the king with the utmost care. Professor Hess, one of the most distinguished of the Bavarian artists, is inspector, and under him there is another accomplished artist, who makes the principal designs, and directs the works.

We have here the secret of the superiority of our neighbours in this and, as I have shewn you, in other ornamental arts. What are our glass-painters as compared with those of Munich? Instead of being accomplished artists, they have hardly a claim to the title at all. We complain of the imperfection of many of our decorative arts, but how can it be otherwise? They are chiefly practised by individuals who, however meritorious, have little claim to artistic knowledge. The establishment of a school of design in every town in the kingdom will not mend this; the use of schools of design is to educate persons who may execute the designs of artists. To make designers, as is expected by many, except for inferior purposes, is impossible. A classical education, a perfect knowledge of the history of art, in fine, an educated mind and a refined taste, are necessary to the designer for important ornamental works; and till it is made worth while for the professors of these to follow the art, and till such persons are employed, we must, of necessity, make what efforts we may, be behind our neighbours in these ornamental arts, since they avail themselves of the services of the finest talent within their reach.

To return to the Munich School of Glass-Painting. The Director first prepares full size cartoons; these he paints in water colour (and I have nowhere seen more beautiful drawings): other cartoons are then prepared which may be termed maps of the colours; these are coarsely executed, but correctly tinted; the simple colours only are indicated; thus a red robe is painted of a flat red, the shades being left out, and so on with the other colours. This map, so to speak, is put into the hands of the glass-cutter; he matches the tints from his stock of coloured glass, and cuts it to the shapes. This process requires much practice; many of the pieces are very small and of somewhat complicated shapes; he must also allow for the leading, or uniting by means of strips of lead, as you see in this example.

An ingenious instrument is used for cutting large portions of circles, of which I exhibit a drawing.

The coloured and white pieces being now united with lead in the usual way, pass into the hands of the artist, and are painted. A mystery is made of the preparation of the colours, and I was not allowed to make any inquiries; but this mechanical part of the art is, I believe, well understood amongst ourselves. After the painting is completed, the lead is taken out, and the pieces of painted glass are put into the stove, of which I have made a sketch from memory. An old man from Nuremberg superintends this department, and is the only person in the establishment who has the requisite experience.

The encouragement given to the art which I have just described, has led to great improvements in the manufacture of glass, and the optical instruments of Munich have now a high reputation.

In France, I was also chiefly occupied inquiring into processes of painting, and I had little time to see to other matters. I cannot venture to offer you more than a few very general observations on the arts of this great and interesting country, but I shall bring under your notice a few facts which bear upon the subject which I have been attempting to illustrate.

Art is extensively patronized in France, and frequently with political views. The patronage, however, is not always judicious, and the very frequent changes of government which have taken place of late years in France has led to this. The patronage of the throne, which is very extensive, has been more steady and has produced great results, whilst that of the ministry has never been on any well-organized plan, and to this most of the faults that have been committed are to be attributed.

Whatever may be the objections to some details of the system, art is extensively patronized in France by the throne, the ministry, and the municipalities, and great has been the progress that has been made.

I might give you many examples of the munificent care with which the French government watches over and promotes the welfare of art; I might describe to you that magnificent establishment, the Ecole des Beaux Arts at Paris; but I must content myself with a brief notice of that most important of all establishments, the French Academy at Rome.

The French Academy occupies the Villa Medicis, with its fine and extensive garden on the Pincian Mount. One of the most distinguished of the French artists is sent to Rome as director, his office enduring for five years; he has fine apartments in one part of the villa, and entertains during winter.

I do not know how many pupils are sent or how long they remain, I believe five years; but this is of no consequence to the view that I am at present taking. The whole cost of the establishment is 100,000 francs, or L.5000 sterling per annum, of which 20,000 francs are expended in travelling expenses during the hot summer months, when the pupils leave Rome (then unhealthy) and visit other parts of Italy.

The students are required to be at home at meals, unless they have the permission of the Director to do otherwise. They are also required to keep regular hours, to be studious and diligent, and to secure this, tasks are allotted to them. The young architects, who study monuments of antiquity, are provided with scaffolding, ladders, and every convenience, and the painters and sculptors are equally cared for; and, lastly, the Director regularly sees company, generally composed from the best native and foreign society, where the pupils have every opportunity of improving themselves in this agreeable and refined intercourse.

Contrast this with the English system. Every three years a travelling student is sent abroad, and he is thrown into a position, for profiting in which he is often wholly unprepared by previous education; he is generally quite ignorant of the history of art, or of any art but that which he has worshipped in the Academy, and is probably prejudiced against all other; he is, in fact, sent to Rome, provided with funds, and perfectly free to follow any course he may choose, without any guide at the very time he most needs one. The result may be anticipated, and I believe that very few of the travelling students have attained to any eminence; whilst, on the other side, many of the most distinguished of the French artists were students in this Roman establishment.

I was assured by the Directors of the Museums in Paris, by M. Ingres, late Director of the French Academy, and by M. Couder, that they considered the French Academy in Rome as one of the most valuable and useful of their national institutions for the benefit of art.

I have contrasted the French system with ours, and this is one of those unpleasant contrasts that we are forced to make; but still, be it observed, we have in our plan another instance of artists doing what they can for the benefit of art, unaided either by the public or the government of the country.

I am unable, as I have said, to give you any detailed account of the arts and manufactures of France; to do so would also be beside my subject. We have, however, much need to be up and doing. We have admitted the superiority, in point of taste, of many of the French manufactures, but we have claimed for ours great superiority of fabric. This, Sir, in many respects, is fast becoming a fallacy; and as the French have greatly excelled us in taste, they are now, in many cases, equalling us in fabric.

I found in the Italian ports our fine woollen cloths and printed goods comparatively unsaleable, because the French goods were much superior. I was informed, that, in America, their glass manufacture, because of its superior beauty, has the advantage of ours in the market; and I have particularly to mention their astonishing progress in cutlery, in which only a short time ago they were, almost to a proverb, deficient. They now produce various articles, which, in appearance, at any rate, are equal to our own. We ought to take these things into our serious consideration.

But to return to my more immediate subject.

Great additions have lately been made to the Hotel de Ville of Paris; and the public apartments of the Lord Mayor, if I may so call him, have been painted by some of the most distinguished of the French artists, amongst whom I may mention Mon. Vauchelet, to whom two of the rooms have been entrusted. In one he has painted, on pilasters of polished white Scagliola, a series of exquisitely designed arabesques; his subject is continued in the frieze and terminated on the ceiling; the other rooms are appropriately and magnificently decorated also. In this building we have an instance of well-directed municipal patronage of art; and a number of churches exhibit, at the same time, instances of the munificent patronage of the worthy magistracy of Paris.

In the Chambre des Pairs, and in that Des Deputés, and in a number of churches and public buildings, we see the results of the efforts of the French Government. At Fontainbleau, Versailles, and in the Louvre (and I daresay elsewhere), may be seen a prodigious number of works of art executed by order of his Majesty the King of the French. To give you some idea of the extent of the works carried on at Versailles, I may mention that I was shewn by my friend, Monsieur Neveu, the king's architect, 60 large portfolios of the drawings for the works now in progress.

I have endeavoured to shew you, in these brief observations on German and on French art, what are the true causes of the superiority of these nations in the decorative arts.

It must be perfectly evident to you that palaces painted by accomplished artists, must be in better taste, better done, and far more complete and interesting, than those like ours, in which, after the architect has closed his labours, the house-painter and upholsterer alone are consulted. This is so evident, that I need not detain you further with many arguments on this portion of my subject.

But our neighbours go farther in this decorative employment of artists. I saw one of the most able of the Parisian artists designing ornamental tapestries for the palace, to be executed at the Gobelins. You will remember that Raphael did the same, and our finest possession are some of the cartoons which he prepared for such a purpose.

The works of artists are, in fine, the very sources from which all our decorative artists draw their examples, and their jumbles are chiefly made up, from the arabesques of the Vatican, those of the palace of the T at Mantua, and the well known designs of Watteau. We shall have nothing new, not even an appropriate application of what is old, till, as of old, and as now in Germany and in France, the most able artists we can command are employed in such departments of art.

The tradesman need not be jealous of this employment of the artist. There can be little doubt that were artists employed to paint, and to direct the painting of our royal and other palaces, that the taste for such decorations would rapidly spread over the land, and the employment of the tradesman would be increased a hundred-fold; whilst, by the diffusion of taste, and the increase of skill on the part of our workmen, that which is now far too expensive for many to adopt would then be brought within the compass of their means.

By the employment of artists, the taste of the nation generally would be greatly improved, and I have no doubt that we should soon be enabled to meet our neighbours in those wide fields where we confess that they have beaten us, and there can also be no doubt that our commercial prosperity would be thereby increased.

Before concluding this paper, I shall briefly bring before you one plan which would, I think, greatly aid in promoting taste in manufactures amongst all classes.

I have endeavoured to shew you that, without a more general employment of artists, we cannot hope to rival our neighbours. I have also said that, like them, we must open museums to our people, and increase the numbers of our schools of design.

With regard to museums, our attention has been almost entirely directed to the accumulation in these of precious works of art and of antiquities, but no one seems to have thought of the accumulation of specimens of ancient industrial art, and also of foreign manufactures. To make artists, and to cultivate taste in the fine arts, we have purchased the Elgin and other marbles, and we are slowly forming a national gallery. We do not think it necessary enough to provide our young artists with casts of portions of statues, and with prints from pictures; we procure for them real Greek and Roman statues, and real pictures by the great masters, and we do well. Now, Sir, I would propose that we should carry out this principle in our efforts to promote taste in manufactures of every description; let us form museums in which the citizen may see, free of all cost, specimens of ancient iron-work, wood-carving, glass-painting, and of such arts as were successfully practised in ancient times, of which examples still exist, and to this museum let there be yearly added specimens of the novel and tasteful inventions of our clever neighbours and rivals, so that we may have an opportunity of knowing what they are about, and of comparing our efforts with theirs.

I was influenced by such thoughts as these, when I purchased, for the Honourable Board of Trustees for Manufactures, the objects which they have obligingly permitted me to exhibit to you. I have formed the nucleus of such a museum as that which I advocate. You may observe amongst these, specimens of iron-work, centuries old, which (as I have already remarked) excel in taste and workmanship all the productions of our days; also a lock of ancient date, which I am told is a miracle both of workmanship and inventive skill.

There are also a few specimens of carving, of ancient glass-painting, and some of tasteful modern German manufactures; there are not many of these, but they are all pleasing examples of taste and skill, and I shall, by their exhibition in our school of ornamental design, give the pupils a better idea of the various arts exemplified in these specimens, than if I were provided with all the prints that ever were published, and were to lecture from morning till night every day of the year.

I have briefly attended to a plan, for which I trust to have the support of your opinion, as one which will be greatly conducive to our advancement in the arts.

CHARLES H. WILSON, F.R.S.S.A.

EDINBURGH, 28*th March* 1843.

Remarks on Natural Terraces or Raised Beaches observed in Scotland, particularly in the course of the River Tay. By CHARLES MACLAREN, Esq., F.R.S.E.

In reasoning on the subject of Natural Terraces or Raised Beaches, we start from a few fundamental propositions:—

1. The transported and water-worn materials found on the top of the highest mountains, assure us that they have all been at one time covered by the sea, and must have been raised above it in one way or another.

2. If the land rose from the sea with a *constant* and *uniform* motion, the action of the water would disintegrate the rocks, cutting away the softer parts and leaving the harder prominent; the broken fragments, sand, and clay, so produced, would form an *equal and uniform coating of alluvium* (soil) *on parts similarly situated*—thick, of course, in all the valleys, and thinner on all the moderately inclined surfaces, while the salient and precipitous rocks would be left bare. The depth of the coating would also be affected by the presence or absence of currents.

3. If the land rose with an *unequal*, or let us say an *interrupted*, motion, marks of the longer action of the water would be visible at those parts where the upward motion was arrested for a time. The long presence of the water at such places would be indicated, first, by indentations on the rocks against which the tides beat; secondly, by the increased deposit of alluvium, consisting of the gravel, sand, and clay produced by the chafing of the water on the rocks, or carried down by streams, collecting just under the level of low water, and forming a projecting shelf or terrace, such as may be seen in many of our Scottish lakes. We have examples of ancient terraces thus formed in the parallel roads of Glenroy, and in the two raised beaches described by M. Bravais on the coast of Lapland.

4. The terraces would be broadest and best marked at the mouths of rivers and streams, or in cavities and recesses on their banks, because the materials which form them are there most abundant.

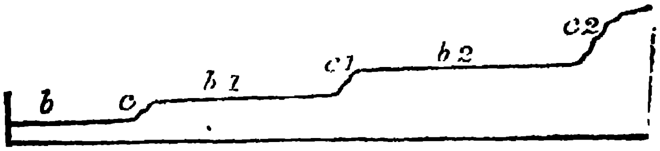
I was highly gratified to observe a number of terraces at different elevations on the banks of the Tay, and generally very unequivocal. They may perhaps have been described before, but the descriptions have not fallen in my way.

The first and lowest terrace constitutes the South and North Inch and the flat ground on which Perth stands. The North Inch, to which my observations were confined, is a plain, and almost a dead level; I estimated its height at ten feet above the Tay at full tide. It has the appearance of an ancient beach, or the outer margin of the former bed of the sea, over which the tides had played for a long period.

The Inch is bounded on the west and north sides by a well-marked acclivity, only a little rounded, which seems to be an ancient sea-cliff—a portion of the shore against which the tides beat, previous to the last elevation of the land.

Behind the sea-cliff is a second plain, and, like the former, almost a dead level. I estimated its mean height at 12 feet above the North Inch. The road to Dunkeld passes through it. This was evidently a portion of the bottom of the sea at a still more ancient epoch.

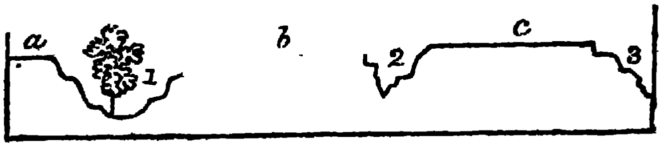
It is bounded on the west side by acclivities so sharply cut at some places, that the water, one might suppose, had washed their bases only fifty years ago. The ancient sea-cliff runs in a sinuous line, jutting out in salient capes or peninsulas at some parts, and receding into little bays at others. It is well seen at Tulloch Bleachfield, and some places near it.



In the above section, *b* represents the present beach near Perth, or that part of the bottom of the sea which is dry at low water; *c* the sea-cliff, or the bank of sand and gravel forming its boundary; *b* 1 the Inch, or ancient beach of the first epoch; *c* 1 its cliff or boundary; *b* 2 the ancient beach of the second epoch; and *c* 2 its cliff or boundary.

The country, for some miles northward, is uneven, consisting of small flattish knolls, rising from four to eight yards above their base, which may be the remains of one or more ancient sea-bottoms, broken up by strong currents. The materials are invariably sand and water-worn gravel, except at a few spots where the rock protrudes.

Near Luncarty we meet with what I am disposed to consider as remnants of an ancient sea-beach or sea-bottom. They are insulated platforms of sand and washed gravel, from 20 to 50 or 100 yards in diameter, flat on the top, and very nearly of the same height. They can scarcely have less than 100 feet of elevation above the Tay at Perth. They may be considerably higher. Several of them are near each other, and merely divided by fissures with steep sides, from 10 to 40 feet in depth, precisely such as little currents would cut out in a bed of alluvium. See the section below.



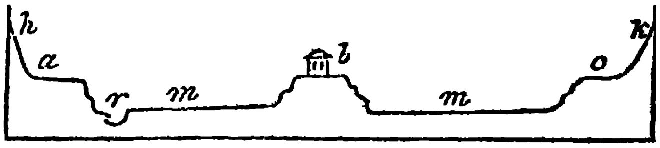
In this figure, *a b c* are three platforms near each other, and of equal height. They are divided and bounded by the fissures, 1 2 3. In some of these fissures, as 1, I was able to judge of the depth from pretty large trees being nearly concealed in them. All these elevations, as already stated, are of sand or fine gravel. I saw marks of stratification in some, and have no doubt that they exist in all.

At Dunkeld, and on the banks of the Tay, far above it, I was delighted to find terraces so distinct and conspicuous that no geologist, who had once turned his attention to the subject, could question their existence.

On the north bank of the Tay, eastward of Dunkeld, a plateau or terrace extends three-fourths of a mile in length, and of a breadth varying from 50 to 600 yards. It is almost perfectly level, and is backed by lofty precipices of quartzy slate, against which it abuts as sharply as the waters of a lake do against the mountains which confine it. A farm-house or hamlet, called Haughend, stands at its inner corner, and the picturesque villa of Doctor Fisher occupies its western extremity. It terminates toward the river in a precipitous bank of sand and gravel, about 120 or 130 feet above the stream.

On the other side of the river, and right opposite, is an isolated hill of sand and gravel, called Tor Hill, with steep sides, and with a villa upon its summit. The top of this hill is a few feet under the plateau of Haughend. It stands in the middle of the valley with low ground on both sides of it; its length, as might be expected, is in the direction of the stream; and if the water rose thirty feet above its present level, the hill would be an island. There is probably a nucleus of rock under the sand and gravel.

On the same side of the river opposite the Cathedral, is another remnant of the plateau, projecting from the mountains which bound the valley on the south. It is about half a mile in length, and a furlong in breadth. The farm-house of Claypotts stand upon it. It declines slightly from the mountain, but its surface is otherwise as smooth and uniform as a sea-beach. A detached portion of it covered with wood is within 200 yards of the bridge. There are similar terraces farther up the valley, as I shall state presently, and they all consist of alluvial matter, sand and fine water-worn gravel, such as we find on the sea-beach. The hill of Torwood, and the terraces of Haughend and Claypotts, are not exactly in one line, but uneven portions of the last do in truth extend as far eastward as the two first, and the small difference of position does not affect the argument.



This diagram is a section across the valley of the Tay.

a, the terrace of Haughend, 120 or 130 feet above the Tay at Dunkeld, and probably 10 or 15 more at the east end, owing to the falls in the stream. *r*, the *bed* of the river. The terrace abuts against the steep crag *h*.

b, Tor Hill, an isolated eminence with a villa upon it.

c, the terrace of Claypotts, very nearly of the same height as *a*. It stands forward from the hill *k*, and presents a steep escarpment of sand and gravel towards the river.

m m, the level haugh or holme which skirts the river, rising from 6 to 15 feet above it.

The breadth of the valley measured right across, at the level of these terraces, that is, from *h* to *k*, may be about a mile.

The *position* and *materials* of the two terraces and the hill *b*, leave scarcely any doubt that they are remnants of a vast bed of sand and gravel which filled the valley from side to side, and was deposited at an ancient period by the sea, when it stood much higher than at present, covering the plain of Perthshire up to the Grampians, and occupying the valley of the Tay far above Dunkeld, in the shape of a narrow firth like Loch Etive or Loch Long.

I am indebted to a scientific friend for authentic data which enable me to fix the difference of level between the Tay at Perth and the Tay at Dunkeld in round numbers at 160 feet. Adding 120 for the height of the terraces, and 20 more for the probable elevation of the water above their surface, it follows that the relative level of the sea must have been 300 feet higher when these terraces were formed, than it is at this day.

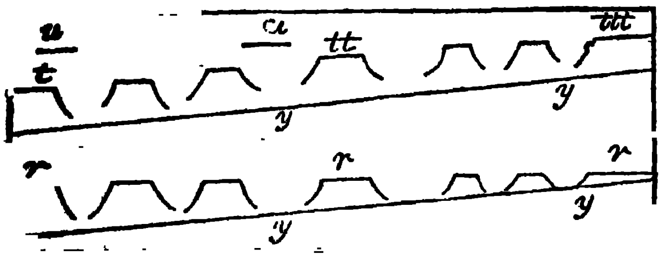
There is a small conical hill within the Duke of Atholl's park, very near the village, which, if not artificial, must be another remnant of the plateau of gravel and sand.

The terraces are not confined to the vicinity of Dunkeld. The road to Taymouth runs along the south side of the river as far as Logierait, and the traveller finds on his left hand, not one or two, but a series of remnants of terraces, like *a* and *c*. They are all of sand and washed gravel; the sand occasionally unmixed, very pure, and stratified. At many parts they project from the hill side into the low *haugh* land, like promontories or peninsulas running into the sea. Their front and sides are in general sharply cut, and steep, indeed, as highly inclined as the nature of the material will admit. A portion extending half a mile along the valley may sometimes be seen, but usually they are much shorter. Their height varies much; and this is what might be expected; for the stream or receding ocean, in cutting away all the middle, and hollowing out deep cavities in the sides, must have reduced the height of many of those parts which escaped destruction, Good specimens of these banks of sand and gravel are seen at Ballaichan and Grandtully, above Logierait, as well as at that village, and all the way down to Dunkeld. Sometimes two may be seen with the river flowing between them, divided by a space a quarter of a mile in breadth, and shewing, by the steep sides they present to each other, that they had once been united. There is a well-marked terrace at Taymouth Castle apparently about 35 feet above the stream; the village of Kenmore occupies a detached part of its western extremity.

From Grandtully down to Dunkeld, the river runs *across* the strata, and the sides of the valley are steep, rugged, and picturesque. From Grandtully up to Kenmore, the course of the river is *along* the strata, and the valley is wider and more tame. The terraces are much more conspicuous in the former than the latter.

The hypothesis which would ascribe such deposits to an ancient lake, seems to me inapplicable here. Let us consider the case of the terraces at Dunkeld. If a barrier of rock existed there, to confine the water, and produce a lake, its position must have been at the outer margin of the mountains, that is, at Birnam Hill, which is about a mile and a half below Dunkeld. Supposing the river to cut a passage here, through the mass of hard quartzy slate, that passage would be of the breadth of the stream (about 400 or 500 feet), or not much greater. But *at the level of the terraces* (for it matters not what it is 100 feet lower), the opening between the hills which confine the valley is certainly not much less than a mile in breadth.

The hypothesis, however, which ascribes the formation to the sea, is not without difficulties. The well-marked terraces along the valley, as far as Grandtully, seemed to me, judging by the eye, to vary in height above the river, from 30 feet to 100 or 120. Now, in the 12 miles from Dunkeld to Grandtully, the rise in the bed of the Tay can scarcely be less than 100 or 120 feet; and if the relative levels remained the same when the sea stood 300 feet higher than at present, the line of terraces beginning with that of Claypotts, 120 feet high, and continued up the valley, should have sunk gradually, and disappeared at or near Grandtully; while the one existing there is certainly not less than 80 feet high. A plane passing along the line of terraces, would not be, I think, horizontal, but inclined; and its inclination would, in a general way, correspond with that of the bed of the river.



In this diagram, the line *y y* represents the bed of the Tay between Dunkeld and Grandtully, rising to the westward at the supposed rate of 10 feet per mile. If the sea deposited the terraces, we would expect to find them nearly horizontal, as *r r r*; and of course declining in their apparent height as we follow them westward. Instead of this, they seem to be ranged in the manner, *t, t t, t t t;* and their height above the river, though very variable, appears to be about as great in the upper part of the valley as in the lower.

The terraces, however, being in detached parts, occasionally with long intervals between them, we may suppose that the western portions *t t t*, are fragments of a second or higher line of terraces. If so, remnants of them (*u u*) should be found somewhere eastward above the others. This is one explanation of the facts, and may be the true one. I did indeed see what I considered traces of a second terrace above the first, and a third still higher, at some places, but they were faint and equivocal; and the proximity of the positions *t, t t*, sometimes for miles, amounting almost to continuity, with an approximation to uniformity in height, is rather adverse to this supposition.

Again, it may be supposed, that, when the land rose above the sea, the movement of elevation was not equable, but greater in the interior than towards the coast. We know that the rate of elevation is variable in Sweden, and have reason to believe that it was so in Britain.[*] The upheaval may have been greater at *t t t* than at *t*. But this does not account for the general parallelism of the line of terraces and the bed of the stream. We would expect the one to be either more or less inclined than the other. I do not assert that their inclinations strictly correspond, but they seemed to me to do so in a general way.

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[*] See Mr Murchison's Address to the Geological Society for 1843, p. 45.

The different levels of the terraces might be accounted for by assuming the existence of an ancient series of lakes in the valley; but I have already shewn that this hypothesis is inapplicable to those at Dunkeld. Neither can we resort to the Glacial Theory for a solution of the problem; for broad deep masses of sand and gravel, with smooth and uniform surfaces, like some of the terraces, cannot be lateral *moraines*. My belief is, that they were deposited by the sea, and formed the lateral portions of the bottom of an ancient firth like Loch Long or Loch Etive. It is not improbable that a careful survey, with the levels accurately taken, would explain the anomalous facts I have adverted to.

The principle laid down by Mr Darwin should be kept in mind in investigating these phenomena,—that, supposing the ocean to be the agent, it depends on a combination of circumstances, the form of the surface, the nature of the materials underneath, the presence or absence of currents, &c., whether any terraces are formed, and if formed, whether they are preserved.

There is a pseudo-terrace accompanying the river, which remains to be noticed. A border of flat meadow-land is found here, as in other Highland valleys, on one or both sides of the river. It varies in height from one yard to five or six. It follows the ever-changing level of the Tay, and we can scarcely avoid considering it as produced by the river itself, probably in working its way down through the mass of gravel and sand from its ancient and higher level to that which it now occupies. Streams which meander, and have beds composed of alluvial matter, level the bottoms of the valleys in which they flow, by a slow but constant action on their banks. It consists of a process of erosion and deposition going on simultaneously—sand, clay, and gravel, being constantly removed from the upper sides of the peninsulas enclosed in the flexures of the stream, and laid down on their lower sides, so as to give these peninsulas a very slow progressive motion towards the sea. The whole surface between the high lands on the two sides, having been repeatedly subjected to this levelling process in the lapse of ages, acquires the appearance of a stripe of flat meadow-land, the height of which may be accidentally raised from time to time by the new matter deposited during extraordinary floods. It is possible that the lowest terrace at Perth—I mean the Inch—may be of this description.

I saw no terraces on the sides of Loch Tay, except towards its west end. Near the road about two miles north of Killin, I saw what I considered a terrace, though not very well marked, which, measured by the sympiesometer, I found to be about 240 feet above the loch; and it will probably be about 650 feet above the sea Traces of others were observable at higher elevations, but rather equivocal. They consisted of small areas of sandy soil, nearly level, with small raised banks, partly of earth, partly of rock, behind them.

An ancient beach, from six to twenty feet above high-water level, with a water-worn sea-cliff behind it, is seen on the shores of the Clyde, at Greenock, Gourock, and other places. It is beautifully exposed in Rothsay Bay, and round to Ascog, and is conspicuous on the east coast of Arran.

The soil on all the highland mountains which I visited, from Dunkeld to Inveraray, was of sand like sea-sand, with a small mixture of rolled stones. This is in all probability the same deposit with the *third* or highest alluvium of the Lothians. There are deep beds of the same substance in the valleys, with huge fragments of rocks mixed with them, or resting on their surface. Some of these fragments measure three or four yards each way; and must weigh 50 tons or more. They are generally angular, and belong to the rocks in the neighbourhood. They may be considered, I think, as representing the *second* alluvium of the Lothians, or the clay with angular stones. Of our lowest alluvium or stiff blue clay with rolled stones, I did not observe a vestige; and I saw the superficial deposits in contact with the rock in so many instances, that if it had existed in the mountainous tract between Dunkeld and Inveraray, I think it could not have escaped me. One characteristic of the diluvium in the low country is, that it contains far-travelled fragments of gneiss, for instance, chlorite slate, and even granite, which, in consequence of the long journey, are always much rounded; and when portions of the diluvium are destroyed, these blocks are found loose on the sea-shore, and in the channels of rivers. In the long line from Taymouth to the Trossachs, I looked carefully for blocks of granite, as the only ones to which a distant origin could be safely assigned; but among the myriads of fragments, large and small, in Glendochart, round the head of Loch Earn, and along Lochs Lubnaig and Venachar to the Trossachs, I did not discover a single piece even of one foot diameter. I renewed my search at Inveraray with the same want of success. Yet Inveraray is but fifteen miles from the granites of Ben Cruachan, and Glendochart is about the same distance from those of the Muir of Rannoch. We know that masses of primary slate, from Perthshire or Argyle, have crossed the Ochil or Campsie Hills, and reached the Lothians, transported apparently by currents from the west. Are we to conclude that similar currents could not convey blocks over a much shorter distance—from the west side of the Grampians to the east—through some of the openings in the chain? Or, shall we suppose that the Grampians, like the Alps, form a "centre of dispersion"—that, when they rose up by great successive lifts, from the depths of the ocean, the water dislodged by their upheaval rushed off in powerful currents to all points of the compass, carrying fragments of their rocks with it, mixing them with the pre-existing alluvium, which it tore up and redeposited (*remanioit*, as the French writers say), and thus formed our rolled boulder clay? If this were admitted, we might next suppose that the second alluvium, or clay with angular boulders, was formed by the action of tides and currents afterwards, when the whole region was still under the sea; and the upper alluvium of sand and fine gravel last. But this is much too large a superstructure of theory to rest on so scanty a basis of facts.

A copy of the above article was sent to Mr Darwin, whose speculations on the Parallel Roads of Glenroy (see *Edin-*

burgh New Philosophical Journal, No. 54, for Oct. 1839) led him to study the formation of sea-beaches in valleys with much care. He has favoured the author with some remarks in explanation of the difficulty arising from the *inclined position* of the terraces in the valley of the Tay.

"I can positively assert," he says, "from barometrical measurements, and from finding sea-shells on the surface, and embedded, that the sea in retiring from a rising valley does leave sloping terraces on the sides, like those described by you along the Tay. Such sloping terraces occur in Glen Spean, beneath the true parallel roads, and are laid down in Sir T. Lauder Dick's excellent paper. Nor do I think their explanation difficult, on the principles adopted by you, although, at first, I remember thinking it not obvious. The case, I believe, is this, —that in places where detritus is freely brought down, as where a river or torrent enters the head of a creek of the sea, the effect of an *equable* rise of the land, owing to the check given to the transport of the detritus at an infinite number of successive levels, will be to produce a smooth slope, the inclination of which will be regulated by the amount of transported matter, and the form of the underlying rock. If a central slip, and here and there the sides, were removed, your terraces would be left. The ordinary effect of a period of rest in the elevatory forces, would simply be to render the bottom of the valley flat during such period. There is, however, an early check to any great extension down the valley of these flat places, so as to become prominently visible; for, as soon as the stream has to flow over a small breadth, where flat, it must drop its detritus, fill up its bed, change its course, and so on, until the entire flat surface or terrace is converted into a talus-like slope, regulated by the force of the water in transporting matter from the valley above. As far as my experience goes, it is only when valleys are broad, generally near their mouths, that the sea eats out during periods of rest, the detritus accumulated during a rise, and thus forms cliffs. High up valleys, I believe, it is the river which has removed the central strip, and has left the fringes of sloping terraces. Such fringes extend up nearly every valley in the Cordillera of Chile, to the height of several thousand feet, and are direct continuations of wide plains near the coast, which are undoubtedly of marine origin."

Mr Darwin refers to Sir T. Lauder Dick's paper in volume 9th of the Edinburgh Transactions, and to his own remarks at page 58 of his Memoir on Glenroy, in further illustration of the subject. I have looked into both. Sir Thomas describes, at p. 37, four *inclined* shelves in Glenroy. They are far under the *horizontal* shelves or parallel roads; and, as he states, they are not properly on the side of the hill, like the latter, but between the hill and the river, and the lowest is most inclined. He notices similar shelves in Glean Spean, p. 41.

The difference between the two deposits seems to be this. In a narrow valley, *a horizontal shelf* like one of the parallel roads *is the product of the sea alone* operating on the loose matter brought down by rains, or eroded from the rocks by the action of its own waves, during a period of rest when it stood *at one level.* On the other hand, *an inclined shelf is the product of the sea, modified by the action of a large river* flowing through the valley, and bearing detritus; and it presents the sum of the operations of the sea and the river at *a succession of different levels,* when the sea was subsiding. Each of the inclined shelves in Glenroy is a remnant of a deposit which once extended across the valley in a curved line, and formed its bottom. The parallel roads never extended across the valley, but were originally, what they are still, mere fringes of loose matter at the margin of the sea.

It is, indeed, obvious, on a little reflection, that a continued and *equable* subsidence of the sea would leave an alluvial deposit in the bottom of the valley with a surface pretty regularly inclined. It is only the case of an *intermittent* subsidence that presents any difficulty, and of this difficulty Mr Darwin has offered a solution which is at least plausible. The floods in a river like the Tay would go far to obliterate irregularities of outline as they arose, by sweeping off prominent parts and filling up cavities.

Report of the Researches of M. Agassiz during his two last sojourns at L'Hotel-des-Neuchâtelois, upon the Lower Glacier of the Aar, in the years 1841 *and* 1842. By M. E. Desor.

(Continued from page 178.)

1. *Experiments on Infiltration.*

The theory of infiltration admits that the water produced by superficial melting does not altogether escape by the internal cavities or the terminal openings of the glacier; and that a considerable portion of it penetrates into the interior, is there congealed, and so contributes partially to supply the loss which the glacier undergoes at the surface. This water percolates throughout the whole mass of the glacier, by means of a general net-work of capillary fissures, which fill the larger basins, the hollows of pools, cascades, &c., in the same manner as the minute blood-vessels supply the larger trunks in the living body. This net-work of minute fissures is so evident at the surface, that it admits of no doubt or dispute; but, as formerly remarked, the opinion that the same arrangement pervades the whole mass, has been much contested. Hence it became necessary to devise some method whereby the fact might be ascertained; and this, it appeared, might be satisfactorily done by the introduction of coloured liquids into the ice: for, if the dye coloured the ice to a certain depth, a proof would be afforded that the capillary fissures reached to the same extent. Accordingly, M. Agassiz brought along with him a barrel filled with a highly concentrated infusion of logwood, kindly prepared by M. Henri Du Pasquier, and also a quantity of chromate of potash, both of which are intense colours, without being expensive. We began by boring at the margin of a canal cut by the running stream near the *Hotel-des-Neuchâtelois* a couple of holes about two feet deep. Into each of these we poured about a quart (*litre*) of the infusion of logwood, and in half-an-hour we perceived that the wall of the ice was discoloured in front of one of the holes, and to the depth of a foot beneath the lowest part of the excavation, and ere long the dye percolated from all the capillary fissures. In two hours, all the infusion had thus strained off. The same evening, the other hole also was quite empty, and the wall of the ice, in front and beneath it, dyed in a similar manner.

This, however, was not an unexceptionable experiment; for it might with truth have been objected, that, granting that the fissures extended so far, still it was nothing more than the result of the atmospheric influence. To meet this objection, it was proposed that we should bore perpendicularly at the margin of a cleft, as deep as 15, 20, or 30 feet, and then introduce the coloured liquid. But even then it was possible that the liquid might escape by the sides of the bore, and it might have filtered vertically, where it would have been impossible it could be observed. It was, moreover, proposed to cut a gallery not far from the margin of the glacier; this, however, would have been very laborious and tedious, and would likewise have been exposed to the objection brought against the experiment already detailed, on account of the alleged difference of the state of the ice near the surface and in the interior. At length we discovered, upon the branch of the Lauter-Aar distant about a league from the Hotel, and near the place where the transversal band, 500 feet long, was measured—an arrangement which seemed admirably adapted for our experiment. Here we found two great crevices, separated by a wall of ice fifteen feet thick, and as both were very deep, and wide enough to allow a descent to the extent of thirty feet and more, we determined to sink a gallery thirty feet, at its lowest portion. Four active hands were immediately put upon it. They commenced by cutting a staircase along the narrower crevice; and after having erected a small scaffold, they proceeded to form a gallery four feet high, and three wide. This was most annoying work, especially when the thaw at the surface was considerable, for then the water dropped from the roof of the gallery in great quantities, and thoroughly drenched those within it. Hence, they were all the better of the mountain breeze and Alpine air when at work. When they had reached the depth of eight feet, M. Agassiz determined, before proceeding farther, to make an experiment respecting the infiltration. A hole, a foot in diameter, was bored directly above the gallery, to the depth of five feet; into this, at mid-day, was poured five quarts (litres) of the infusion of logwood, which all disappeared in half an hour. We had no doubt about the result of our experiment, for the presence of the capillary net-work traversing the entire mass, was now no longer an hypothesis, as the quantity of water infiltrating from the sides of the gallery was a clear demonstration to our senses. In spite, however, of this, it was with much joy that we received the information, two hours and a half after the introduction of the infusion, that it had appeared on the roof of the gallery. We all betook ourselves to the spot, where we clearly saw, at the upper angle of the roof, a large marking of a beautiful yellow colour, which could only have been produced by the infusion of the logwood. Presently we observed the spot enlarge downwards and laterally. We attentively observed the manner of its propagation, and perceived that the coloured water insinuated itself into all the fissures, advancing very much *per saltem.* In removing, with a hatchet, a part of the coloured portion, we distinctly observed that the colour pervaded only the capillary fissures, and that the icy interspaces were themselves quite colourless. After some hours, the tinting had reached as far as the floor of the gallery, and was descending deeper and deeper. Thus we had a convincing proof that the capillary fissures are a phenomenon not of the surface only, since they exist at depths where external agents exercise no influence, and that most probably they extend through the entire mass of the glacier.

These infiltration experiments were often repeated, and always with the same results, the coloured liquid usually reaching the roof of the gallery in less than two hours. Whilst narrowly examining the circulation of the liquid in the interior of the ice, we were led to many observations upon the modifications and variations in the capillary fissures themselves, which, in general, are much larger than is usually supposed. We also noticed that the *podurellæ* or ice-fleas (*Desoria glacialis*, Nic.) readily found their way into them. We discovered these insects in the gallery the very day it was opened, and we often saw them moving freely about in the interior of the most compact ice, sometimes at the depth of two or three inches.

The phenomena of infiltration were, of all our experiments, the ones which most interested travellers; and hence all our friends who took an interest in our researches were too happy to examine them. The gallery itself presented a curious spectacle, and involuntarily suggested to the mind some crystal grotto of the fairies, surrounded as it was by subterranean and yet resplendent walls. For a time, all our observations were made under the influence of the light of day. We were desirous of likewise observing how the coloured liquid would appear during the darkness of night, and M. Agassiz, accompanied by M. Escher de la Linth, visited the gallery after nightfall on the 1st of August. About 9 P.M. they poured two quarts of the infusion into the hole, and as it was then exceedingly cold (the thermometer indicating—0°. 6, or 30°. 9 F.), they scarcely hoped for a satisfactory result. They imagined that the infusion would congeal before it could penetrate far; and therefore, were all the more agreeably surprised to perceive the coloured liquid on the roof of the gallery in five minutes. Thus, it was demonstrated that the external cold did not hinder the infusion from circulating in the capillary fissures, since it penetrated with more velocity than during the heat of the day. Hence, since the nocturnal cold did not sensibly influence the temperature of the ice, it was easy to explain the accelerated movement of the liquid during the night; for this simple reason, that as the fissures received less of the surface-waters for several hours, they became empty, and consequently the passage of the infusion was facilitated. The glacier might thus be compared to a sponge from which we have squeezed out the water, and which, of course, is all the more ready to receive more. The fissures, themselves, are also much more distinct during the night, and, with the light of a candle, rather than with that of day. In these circumstances, they can be traced in the walls of the gallery to a depth of about three feet. A light placed in the gallery could be seen very distinctly through the opposite wall, through a thickness amounting to eight feet and more.

From this fact, an important deduction follows, namely, that if the infiltrated water congeals in the interior of the glacier, and thereby determines its movement, this congelation is not owing to the nocturnal cold, as was believed both by M. Charpentier and by M. Agassiz. On this point, the result of our observations last year quite agree with the opinion of Professor Forbes: a subject to which we shall ere long return. But our experiments upon infiltration were not confined to the hard and compact ice of the glaciers, properly so called; they were also repeated upon the nevé and upon the ice of the nevé, and with results equally satisfactory. The true nevé, composed of incoherent snow particles, could be impregnated with water in no other than in a uniform way. But it is more important to observe, that the adhering or connected nevé, in other words, that dull and opaque ice which forms the substratum of the nevé, and which we call the *ice of the nevé*, imbibes water in precisely the same manner. In it we find no capillary fissures as in compact ice, serving as canals to the infusion; but it spreads throughout the whole mass, almost as in a porous rock, and with much greater rapidity than in the ice of a glacier, properly so called. Moreover, the compact ice itself also presents very marked differences in the rapidity with which it is stained by the colouring liquids. It often happened that we bored many holes near each other, and observed that the liquid was absorbed in some in a very short time, whilst it remained much longer in others. Struck by this difference, we investigated its cause, and discovered that those holes which rapidly became empty had been sunk in bands of blue ice, whilst those which long retained the impression were bored in the white ice. Hence the difference clearly appeared to be a natural consequence of the structure of the blue ice, which is traversed with many more fissures than the white. In this particular, we find we are directly opposed to the statement of Professor Forbes, who mentions that it is the white bands that especially subserve the purpose of infiltration. It is true, that in proportion as the white ice loses its air-bubbles, in other words, becomes water-ice, *glace d'eau*, by the infiltration of the surface-water, its network of fissures increases, and finally it allows the transudation of the liquid as rapidly as the blue ice; but this occurs only in the lower regions of its descent. The ice of the gallery afforded us an example of this transformation; for here we could but imperfectly distinguish between the white and the blue ice, and the liquid everywhere infiltrated with the same rapidity. We have found that the chromate of potash is preferable, for these experiments, to the infusion of logwood, however strong it may be; the chromate, moreover, is of infinitely easier carriage, and dyes more deeply, especially when mixed with a little acetate of lead. With a quart of this solution we have coloured a considerable stream to the extent of many hundred feet.

From these experiments, it follows, that water transudes through ice throughout the whole extent of the glacier; that this transudation takes place in different ways, and with a velocity varying in different places; that in the nevé the transudation is uniform in its progress; that in glacier ice, properly so called, it is effected by means of a net-work of capillary fissures, which extend as far as we can penetrate, and probably throughout the entire mass; and, finally, that in the middle region of the glacier, in which the differences between the white ice and the blue, which may be called the ice of infiltration, are still marked, the transudation occurs much more rapidly in this latter than in the former; and in proportion as these differences are lost, the infiltration takes place in a uniform manner.

These results are fully confirmed by the following observations. When we remark, after a cold night, the different hollows full of water, and which exist in great numbers at the surface of the glacier, we usually find the surface of the water covered with a pellicle of ice, which disappears as the day advances. In the morning this pellicle does not repose immediately upon the water, but is separated by a space, which varies from half an inch to two inches, and even more. It begins to form immediately after sunset; and often we see spiculæ of ice on the surface of the pools as early as six o'clock. The ice thus protecting the water against evaporation, there remains but one means of explaining the diurnal lowering of its surface, namely, the admitting the infiltration into the interior. The same consequence results from the observations we have made upon the great reservoirs. When we arrived at the glacier of the Aar, at the beginning of July 1842, all the crevices, as well as the hollows of the older cascades, were filled with water even to overflowing; after a few days of fine weather, we readily perceived that the water had sunk; and shortly afterwards it had so considerably diminished, that it was quite impossible to attribute the fall of the surface to the effect of evaporation alone.

It would have been important to have ascertained how long the water which is engulphed in the glacier near the *Hotel des Neuchâtelois* takes ere it is discharged at the terminal vault. For the accomplishment of this desirable object, various experiments were proposed, some of which were in principle mechanical, and others chemical; that mechanical method which appeared most simple, consisting in throwing into a cascade some bodies, such as minute balls of wood, or saw-dust, which would float with the stream. The saw-dust from the oak appeared the most suitable, on account of its specific gravity, which very nearly corresponds with that of water; although there was much reason to fear that it would but too readily adhere to the sides of the ice it encountered. The chemical method would have been preferable on a small scale; but it was very doubtful if any re-agent, however energetic, could detect the presence of a solution in such an immense mass of water. Notwithstanding, we often tried it. M. Agassiz desired two sacks of saw-dust to be transported from Meyringen to the glacier, which were there thrown into a cascade or *moulin*, at a certain hour in the morning; but we did not see the slightest trace of it at the extremity. On another occasion, 30 quarts (*litres*) of the infusion of logwood were poured into the same cascade, without affording a more satisfactory result.

2. *On the Blue Bands, or those of Infiltration.*

Our attention, when continuously directed to the structure of glaciers, could not but be attracted to many particulars which are produced by the transformation to which the ice is subjected in the progress of its course. Long ago we had observed on the surface of the glaciers long rectilinear and parallel fissures, which were remarkably numerous in the vicinity of the morains. The first explanation which occurred was, that these fissures were crevices. Their shallowness, however, their frequency, and the remarkable peculiarities with which they were accompanied, rendered this explanation by no means probable. In the year 1838, M. Guyot remarked on the glacier of Gries remarkable alternations in the condition of the ice, alternations which seemed to correspond to these parallel fissures, or rather to be only a modification of them. He noticed a succession of laminæ of different appearance, which seemed vertically arranged, and some of which were harder than others, so that they projected somewhat beyond the others. M. Guyot communicated these observations verbally to the Geological Society of France, which met at Porrentruy in the month of September 1838. The following is an extract from his memoir.

"Since the word stratum has escaped me, I must here point out to future observers a fact upon whose explanation I will not venture, seeing I have witnessed the phenomenon but once. It occurred at the summit of the Gries, at the height of about 7500 feet, a little beneath the line of the Firn, or high nevé, where the ice passes into the state of granular snow. The glacier at this height exhibits a vast sea of ice, descending from the west in an almost imperceptible slope from summits which are not very salient in appearance; it covers the entire surface with a covering of uniform and indivisible ice, more than half a league wide, traversing the mule-path which leads from the high Valais, by the Vale d'Egine, into the valley of Formazza, and to the Lago Maggiore. At the origin of these two latter valleys, the ice, still half snowy, diverges northward to form the beautiful glacier of Gries, properly so called, and to the south to form the much smaller glacier of Bettelmatten. In ascending to the origin of this last, that I might more narrowly examine the nature, formation, and deviation of the great transversal clefts, I observed that the surface of the glacier under my feet was quite covered with regular furrows from one to two inches in breadth, hollowed out in a half snowy mass, and separated by projecting laminæ, composed of harder and more transparent ice. It was here evident that the mass of the glacier was composed of two kinds of ice, the one (that of the furrows) still snowy and softer, the other (that of the laminæ) more perfect, crystalline, glassy, and more resistent; and that it was to the unequal resistance which they offered to the action of the atmosphere, that was due the hollows of the furrows, and the projections of the harder laminæ. After having traversed them many hundred yards, I reached the margin of a great cleft, from 20 to 30 feet wide, which, cutting perpendicularly in the direction of the furrows, and discovering the interior of the glacier to the depth of 30 or 40 feet, allowed us accurately to distinguish the structure of the ice in a most beautiful transverse section. As far and deep as my view extended, I saw the mass of the glacier composed of a multitude of minute strata of snowy ice, separated every one from its neighbour by one of the icy laminæ above alluded to, and forming a regular stratified whole, not unlike to certain schistose limestones. They corresponded on the two sides of the cleft, exactly like the strata on the opposite sides of a transversal valley."

All who had any accurate knowledge of glaciers were aware that this phenomenon was strictly allied to the superficial fissures which I have alluded to above; at the same time, as they were ignorant of the cause, they were content to register it among those numerous facts which still required elucidation. M. Agassiz, in his *Etudes sur les Glaciers*, speaks of it only casually; nevertheless, our attention was often directed towards these remarkable appearances during the sojourn we made upon the glacier of the Aar, in the month of August 1840; and we were often astonished at their regularity, remarking, that they scarcely commenced sooner than at a league above the extremity of the glacier. I well remember, that often, when walking with M. Nicolet in the neighbourhood of our hut, I used the blade of my knife to satisfy myself that the fissures had really in the interior that remarkable continuity which characterizes them upon the surface of the glacier.

In 1841, the epoch at which we commenced to study in detail the different phenomena which the glaciers present, the longitudinal fissures, and their actual relation with the bands mentioned by M. Guyot, could not fail to attract our attention.

Professor Forbes, upon the invitation of M. Agassiz, joined us at the commencement of the campaign, with the intention of studying, along with us, the different phenomena in general physics which the glaciers present. The parallel fissures often constituted, during the three weeks we together inhabited the *Hotel des Neuchâtelois*, the subject of our communings and our discussions; they appeared to us much more conspicuous than in the former years, and when they were examined at the margin of a crevice, it was distinctly seen that they corresponded to blue bands of a very deep tint, which penetrated as far as the eye could follow them into the crevices. They were seen equally distinctly in the bed of all the streamlets which flowed near to the *Hotel des Neuchâtelois*, and, in general, in all submerged places. The small space cleared of the rocky debris which separates the two portions of the middle morain at the *Hotel des Neuchâtelois*, displayed it in abundance; and the phenomenon was seen in perfection when we cleared away a part of the moraine. We even remarked that it was under the moraine, and in its immediate neighbourhood that the blue bands were largest and most numerous, so much so that in certain places the glacier appeared to be really composed of immense plates of glass parallel to each other, and in juxtaposition. Hence we never failed when any of our friends paid us a visit to clear away a corner of the moraine to shew them the blue bands, and many of them were so surprised that they imagined it was a phenomenon which we had artificially produced. The bands are of variable width, and their number in different parts of the glacier varies no less. Some are ten inches or a foot broad; others scarcely a line. Upon the whole, the white prevail greatly over the blue bands, although there are places in which the latter occupy nearly as much space as the former. When first exposed, the bands of blue ice are perfectly transparent, and the eye can penetrate many feet deep. But this transparency continues but for a very short time, and speedily minute cracks are perceived, at first superficial, but soon they form a net-work, and deprive the blue ice of all its transparency. These cracks also pervade the white bands; and on approximating the ear to the ice, we distinctly hear a slight noise of crepitation produced at the moment of their formation. We were naturally curious to know if these blue bands penetrated far into the interior of the glacier, and it was his interest in this point that led M. Agassiz to descend one of the pits of the glacier to the depth of 120 feet. I shall extract the following passage from his paper in the Edinburgh Philosophical Journal, which relates to his descent, which we were in the habit of calling his *descente aux enfers.*

"It was toward the termination of our residence on the glacier, when we had finished our boring, and were preparing to depart, that, while discussing the phenomena we had observed, one of the party remarked, that it would perhaps be easy to descend, without danger, into some one of the pits of the glacier, and that, perhaps, some unexpected appearances might thus be observed. We all joined in this opinion, and without delay commenced seeking for a pit suited for the purpose. These pits, as I have remarked in my ETUDES SUR LES GLACIERS, are probably old crevasses, which a small stream of water has prevented from being completely closed; so that, instead of being of an elongated form, they are, on the contrary, for the most part circular, and the rivulet, far from contracting them, tends, especially when considerable, to enlarge them more and more. We found one of these pits at some distance from our hut, which seemed well adapted for our object; its mouth had a diameter of eight feet, and it seemed to penetrate vertically to a great depth. I resolved, accordingly, to descend; and to accomplish this, it was necessary, first, to cut off the stream, by making another channel for it. We set all hands to work; and when the new bed was formed, I sent my men to procure the *tripod*, which had been used for the boring operations, and placed it over the pit. A board, on which I was to sit, was fixed to the end of the rope, and I was secured to that rope by a strap, which passed under my arms, so that my hands were left free. In order to protect me from the water, which we were not able to turn off completely, the guides covered my shoulders with a goat's skin, and put a marmot-skin cap on my head. Thus accoutred, I descended, provided with a hammer and a staff. My friend Escher was to direct the descent, and for this purpose he lay forward on his face, with his ear hanging over the side, the better to hear my directions. It was agreed, that so long as I did not request to be drawn up, I should be allowed to descend, as far at least as M. Escher could distinctly hear my voice. I reached a depth of 80 feet without encountering any obstacle, attentively examining the lamellar structure of the glacier, and the small stalactites of ice, which were attached on all sides to the walls of the pit. These stalactites were from 2 to 5 or 6 inches long, and only a few lines in diameter; and they were bent like hooks fixed in the walls. It was evident that they were produced by an exudation from the walls of the pit; for if they had resulted from the water falling from the surface of the glacier, they would not have been so uniform, nor so equally distributed over the whole surface of the sides. Those which were really derived from the cascade of water from above were much larger, were more closely united to the wall of the ice, and were, moreover, limited to one of the sides of the passage. The bands of the blue ice became perceptibly broader as I descended; they were less sharply marked than above, and the remainder of the mass, of an inferior degree of whiteness, was less distinctly contrasted with the intermediate deeper coloured laminæ. At a depth of about 80 feet, I encountered a ridge of ice which divided the pit into two compartments, and I endeavoured to enter the widest; but could not penetrate more than 5 or 6 feet, because the passage became divided into several narrow canals. I caused myself to be raised a little, and making the rope deviate from the vertical line, I got into the other compartment. I had observed in descending, that there was water at the bottom of the pit, but supposed it was at a very great depth; and as my attention was especially directed to the vertical bands, which I continued to trace by means of the light reflected by the brilliant walls of the ice, I was very much astonished when I suddenly felt that my feet were immersed in water. I immediately directed that I should be raised, but my order was misunderstood, and I found I was descending instead of ascending. I then uttered a cry of distress, which was heard, and I was raised before it was necessary to have recourse to swimming. I felt as if I had never before encountered water so cold. Fragments of ice floated on its surface, consequently pieces of broken stalactites; and the walls of the pit were rough to the touch, doubtless owing to the capillary fissures.

"I should have wished to have remained somewhat longer, to have examined in detail the structure of the ice, and enjoyed the singular spectacle of the blue sky, as seen from the bottom of the abyss; but the cold obliged me to ascend as soon as possible. On reaching the surface, my friends informed me of their anxiety for my safety on hearing my cries, and of the great difficulty they had experienced in drawing me up, though they were eight in number. I had, however, reflected but little on the danger of my position. Perhaps, had I previously known it, I should not have so exposed myself; for, if one of the sharp-pointed flakes of ice lining the walls had been detached by the friction of the rope, and had struck me in its fall, the danger would have been great. I would, therefore, advise no one to repeat the experiment, unless for some important scientific purpose."

Like many other natural phenomena, that of these blue bands, in other words, the ribboned structure of glaciers, gave rise to many singular suppositions; and this tendency to imagine extraordinary causes for the explication of facts, which are for the first time observed with attention, is very remarkable. Without doubt, the ice of our lakes and rivers exhibits no appearance of this sort; and we should not be astonished at this, since the origin of glaciers is wholly different from that of common ice. If we follow one of these bands throughout a more or less considerable extent, we speedily discover that they invariably grow narrower, and become more rare as they ascend towards the nevé. It is at their origin, then, that we must examine them attentively, ere we shall be able to distinguish the causes which produce them. In 1841, M. Agassiz expressed the opinion, that the blue bands are bands of ice formed from water, in the midst of the white ice which is the product of the nevé; and he thus conceived that they constituted one of the means by which the water infiltered into the interior of the glacier, and so produced its movement. Professor Forbes, on the other hand, in the account of his researches of 1841, simply compared them to the cleavage of rocks. This comparison, however, explains nothing, and there is not much analogy between a mass of rock and a glacier.

During the winter of 1841–2, these blue bands gave rise to keen discussions ; and hence were an object of our marked attention during our expedition of the summer 1842. In fact, we were scarcely settled again at the *Hotel des Neuchâtelois*, when every one commenced to collect the whole of the facts, for the purpose of explaining the phenomenon in the most satisfactory manner. Numerous observations have, in every respect, confirmed M. Agassiz's first suppositions concerning the origin of the blue bands, namely, that they are simply bands of pure ice produced from water—an account which, at the same time explains why these bands are especially numerous near the moraine, where the glacier is subjected to the most rapid thawing. We have not space here to produce all the observations and experiments upon which this explanation is grounded; and, therefore, we refer our readers to the work of M. Agassiz, which is in the press, wherein this question will be fully considered, and accompanied with plans and plates, which will facilitate its comprehension. I shall only state, that to arrive at an accurate knowledge of their distribution, M. Agassiz caused a trench to be cut a foot wide across the whole breadth of the glacier, extending to 4000 feet, and to a depth sufficient for the distinct examination of the bands, even in those localities where the surface was most disintegrated.

This trench, opened at about a league from the *Hotel de Neuchâtelois*, was included in the transversal band of 500 feet, of which we shall say more hereafter. It results from the observations, made with the greatest care by M. Vogt, and traced on a scale of one-tenth of the natural size, not only that the bands are much more numerous and broader under the moraines and in their neighbourhood, but also that there are very marked differences between the right and the left banks—differences which evidently result from the position of the valley in relation to the sun. Upon the right bank, which is protected from the solar rays by the Grünberg, the bands are fewer and more distinctly circumscribed than on the left, where the whole mass is, as it were, more advanced in its development. For the same reason also, we find the bands farther down distinct on the right side, when none exist on the left. In a word, the bands disappear at the distance of a league from the extremity of the glacier ; or, at all events, we very seldom find them distinct farther down. At the terminal extremity, the whole mass is, as it were, transformed into blue ice, and it is the ice full of air-bubbles, or the white bands, which are the exception. As the lamellar structure disappears, the striæ seem to assume a certain leaf-like appearance, which must not be confounded with the ribboned structure. On the side of the nevé, the blue bands of the glacier of the Aar do not extend much more than a league above the *Hotel des Neuchâtelois*, and Professor Forbes is certainly mistaken, when he alleges they may be traced throughout the whole glacier of the Finsteraar. I have indeed here and there met with some slight trace of them, even above the Abschwung, but they are so slight and irregular, that they must be regarded only as exceptional. Numerous observations have likewise been made upon the direction of the blue bands, and also on their inclination, which, with those regarding the hardness of the different kinds of ice, will be published in detail in M. Agassiz's work.

Professor Forbes, also, in 1842, resumed the study of the blue bands ; and as if the comparison he had made the previous year between these bands and the cleavage of certain rocks constituted an obligation to persist in the same explanation, he assumes this analogy as the starting point of his new researches, and endeavours to convince us, that the blue bands are owing to the friction of two glaciers moving with an unequal velocity ; adding, that between two strata of rock, we often meet with intermediate bands which seem modified by the friction which the former have exercised upon each other. The mere quoting of this explanation is sufficient for its refutation.

3. *On Stratification.*

The stratification apparent in the glaciers is a phenomenon to which all the attention it merits has not hitherto been given.

M. Agassiz, it is true, had announced the fact, that all glaciers are stratified, and had given a figure representing this arrangement in his Atlas; but he had not sufficiently insisted upon the manner of its existence, and of the different circumstances which usually accompany the stratification; and it was not till last year (1842) that it was studied with all the attention it deserved. It is truly astonishing that so important a phenomenon had not been previously investigated. It was only in the higher regions of the nevé in which, with unanimous consent, stratification was admitted. There, indeed, it is so distinct, that, when standing upon the margin of one of those great hollows which are met with in all nevés, you may distinctly count the number of the layers. On this point, I refer the reader to what I have said elsewhere, when describing the immense hollows we observed in the upper parts of the glacier of Viesch, when ascending by the Col de l'Oberaar to the Jungfrau.* As to the strata of glaciers properly so called, many authors, and among others M. Charpentier, formally denied their existence, maintaining that they could exist only in the nevé. The following question, however, as it appears to me, might very readily occur to the minds of the observers who were cognisant of the phenomena of stratification in the more elevated regions, namely, what became of those annual strata, so regular, and so well defined in their superposition? It is true, that when we compare this very distinct superposition with the uniform appearance of the walls of the crevasses in the less elevated regions, where the whole mass is of a proverbial uniformity, we are very naturally led to suppose that these strata must have been necessarily effaced by the successive transformations which the ice has undergone; and it is probably owing to Professor Forbes having confined his attention to these regions, that he fell into the strange error of confounding the strata of the glaciers with the blue bands. In his latter publications, he positively affirms, so far following M. de Charpentier, that the glaciers are destitute of stratification, whilst he has actually witnessed the strata as we have done, and has even given a sketch of them in one of his letters: new proof this how difficult it is to guard against the tendency of our minds to confound phenomena as identical, which have only an external resemblance. It is probable that if Professor Forbes had been less preoccupied with the vast importance of the blue bands which he was in the habit of regarding as his exclusive domain, he would not have hazarded so unsatisfactory an explanation. After having viewed from the height of some commanding summit the out-croppings of the strata, he should have descended to the nevé, and then followed the windings of the same lines he had before observed, compared and sketched when above them, and he would then have soon been convinced that these lines have nothing in common with what he has designated the ribboned structure, answering to our blue lines, but that they are truly real strata. He would have seen, in particular, that at the limit of these out-croppings the strata are distinctly separated, and that he might there readily introduce a knife, or any other slender body, to a considerable depth.

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* See *Bibl. Univ.* November 1841 (vol. xxxvi.), p. 120, et suiv.

Assuredly, nothing is more natural than the presence of these strata or beds in the glacier. Every winter there falls in the higher regions nearly an equal quantity of snow. Towards spring, when the temperature begins to increase, and when alternations of thaw and frost occur, there is formed on the surface of this bed a hard crust, which becomes thicker in proportion as the frost and thaw are more frequent. This frozen crust on which the dust upborne by the winds and tempests of summer descends, separates the bed of the former winter from the covering of snow which falls the ensuing one; whence it results that, in the hollows of the great circuses, all the beds are separated by beds or zones or laminæ of ice more or less tarnished and soiled. As thus the quantity of snow which falls in these elevated regions is nearly the same every year, as remarked by the mountaineers, what more natural than that the superimposed strata should be of nearly the same thickness? The idea that these beds are annual deposits, occurs spontaneously to the mind; it is the opinion generally entertained by the inhabitants of the Alps; and no naturalist that I know has hitherto thought of questioning its accuracy.

We sometimes, nevertheless, observe striking irregularities in the midst of the general uniformity; thus, instead of a bed of seven to ten feet, we find two of from three to four feet in thickness. In this case, we must admit a long interval between the snow-falls of the same winter. It is thus that, during the winter of 1840–41, after the snows of autumn, the environs of the Grimsel, and of the glacier of the Aar, enjoyed throughout the whole months of December and January a mild temperature and a serene sky. The temperature in the warmer days must have been above zero even in the highest regions, and this would have been sufficient to form a superficial crust; separating the snow which fell in autumn from that which fell in spring. Hence we should not be astonished to find at some future period a double stratum corresponding to this winter, when, in consequence of the movement of the whole glacier, these masses shall have been engaged in the common movement.

This explanation of the formation of the annual beds is so natural, that I think it cannot be disputed by any one. Unfortunately, the superposition of the beds is visible in the most elevated positions only, where very few naturalists go to examine them. Lower down the nevé succeeds, which is compact, and here the crevasses are generally too few and shallow to allow our observing the stratification; and as to the glacier, properly so called, it exhibits at its surface only very faint traces of the phenomenon. It is only by ascending to an elevated part of the highest ridges, where we command an extensive view of the whole glacier, and by extending our prospect from these upper regions, where the out-croppings of the beds are very distinct, to the less elevated districts, that we perceive these croppings extend from above downwards, but always less and less distinct as we approach the extremity of the glacier. By thus having the whole of the phenomenon at once under our view, we are easily convinced that the strata exist throughout the whole length of the glacier, however obscure they may appear at first sight. It is in this way much as it is in metamorphic rocks. At first view, no one would recognise beds in those granitic walls which surround *l'Hospice du Grimsel;* but if we ascend one of the environing summits, we are soon persuaded, while beholding the constant direction of the lines, that the whole mass is stratified, though in a manner somewhat obscure.

The outline of the strata is very far from being similar. If the glacier moved in an equable manner throughout its whole mass, we might, without doubt, expect to find all the projecting lines regular; but if the movement occurs irregularly, and the middle part moves more rapidly than the sides, or shall have deep and hollow beds, and if the more rapid movement in the middle continues throughout the whole extent of the glacier, the curves will thereby approximate more and more, and finally will assume the form of a very pointed arch, known by architects under the name of *ogive*. Now, this is precisely what takes place; and it is this which, when it reaches a certain limit, makes it so difficult to recognise the outline of these so greatly prolonged arches or curves.

Another complication presents itself, when the glacier is composed of several portions or tributaries. Each portion at first has its own system of beds, and in the more elevated regions, where the movement is quite regular, the outline of the various portions has a considerable resemblance to that of imbricated tiles. But this regularity exists only in the most elevated regions. Speedily, from local causes, such as the irregularity of the ground, the shape of the valleys, the relative position and size of the different glaciers themselves, &c., the movement ceases to be equal in the different portions; some are more stationary, while the others advance more rapidly, and speedily the one which takes the lead so prevails over the others, that it appears completely to supersede them. The lower glacier of the Aar is very instructive on this point, and the mutual combination of the several tributaries, as they are severally encountered, is one of the most curious phenomena of stratification. An examination of the chart which M. Agassiz is now preparing will illustrate this matter more than the most ample and detailed description.

This, for the present, must suffice, in proof that the glacier is really stratified; also, that the strata correspond to the beds of snow which annually fall in the most elevated regions; that these strata, at first transversal, slowly and gradually become curved in consequence of the more accelerated movement of the middle portion, and that this accelerated movement, continuing throughout the whole length of the glacier, the arches also are proportionally prolonged and straitened, till at last they exhibit the form of very prolonged ogives.

The combination of blue bands, or bands of infiltration, with the strata, is very readily recognised in the upper regions of the glacier, properly so called, where their out-croppings are still sensibly transverse, or but slightly convex; for as the blue bands are usually parallel to the axis of the glacier, they cross the projections of the strata at different angles. But, in proportion as these arcs are elongated, and as their sides become parallel to the direction of the valley, the difficulties increase, and then we can scarcely distinguish the two kinds of phenomena, except after they have been made an object of detailed study. The mode in which Professor Forbes attempts to explain the constitution of the glacier of the Rhone, is an excellent proof of this. It is known that the glacier of the Rhone, about half a league above its inferior extremity, presents a considerable disruption—*éboulement*,—which is known under the name of the Cascade of the glacier, and which is admired by all travellers as highly picturesque. Below this cascade the glacier resumes its regular course, and there is seen on its surface a series of lines or arcs, at first but little curved, and almost transverse, which stretch out more and more, until they describe semicircles, and finally the prolonged arches or ogives. The crevices form a right angle with these lines, whence it results that where these latter are longitudinal (on the sides of the arches), the crevices are transversal; and where the lines are transversal (for example, at the anterior margin of the glacier), the crevices are longitudinal. This remarkable antagonism of the arched lines with the crevices, of which Professor Forbes has given a sketch in the Edinburgh Philosophical Journal,* is so evident, that no one can question it. But the explanation which the learned Professor offers of the phenomena appears to me altogether erroneous: He supposes that a pressure from the descending mass operates in the middle of the glacier, at the base of the cascade, and that this pressure, by forcing forward the concentric arcs, produces the appearance of curves which exists; and because the crevices are at right angles with the arched lines, he concludes that they are the blue bands. Unquestionably, it is a fact, speaking generally, that the blue bands intersect the crevices at right angles, and we willingly concede that Professor Forbes was the first who pointed out the circumstance to the attention of observers; but surely this is insufficient ground for the inference, that all the lines or outlines which exhibit this peculiarity are consequently blue bands. We have a thorough conviction, that if Professor Forbes, instead of confiding in the infallibility of this rule, had more narrowly examined the structure of these concentric arches, he would have been satisfied that they do not at all correspond to the primitive blue bands (his ribboned structure); that these latter are much fewer, and that where they exist they do not coincide with the arched lines of the surface, which are so readily distinguished higher up, at Meyenmand. I believe, that, with a closer attention to facts, we might give a more natural account of all the phenomena. In truth, to every one who notices the arrangement of the strata in a glacier, it is evident that these concentric lines which Professor Forbes regards as blue bands, are nothing else than the outcroppings of the strata. Now, if we admit that the cascade dislocates the glacier only at the surface, there is nothing astonishing in finding that it produces beneath the same arrangement of strata which prevailed above. The only peculiarity which presents any difficulties is the fact, that the arched lines are almost transversal at the foot of the cascade, whilst farther down they form curves which are more and more prolonged. It is as if the glacier was again re-formed after it had passed the fall. Perhaps, also, the strata are but little arched immediately *above* the cascade, and in that case there would be nothing astonishing that their outlines corresponded *beneath* also. The whole difficulty, then, comes to this: Does the cascade break up the glacier throughout its whole depth, or is the whole phenomenon confined to the surface? The examination of the strata at the upper part of the glacier can alone solve this problem.

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* Vol. xxxii. p. 89, January 1842.

4. *Observations on the pretended Purity of Ice.*

Continued researches often lead to the invalidation of opinions which are very generally adopted. It is a prevailing opinion with the mountaineers, and from them has found its way as an undoubted dogma into the works of naturalists and natural philosophers, that the ice of the glaciers is perfectly pure. This alleged purity is even become proverbial in some countries; and from the time of Scheuchzer and De Saussure, down to that of Charpentier and Agassiz, every one was forward to confirm it. When in the clear atmosphere of the high Alps, under the serenest sky, we contemplate those walls and vaults, whose lustre rivals the bright azure of the firmament, we readily persuade ourselves that the ice which composes them must be perfectly pure. And notwithstanding, if we reflect upon it, we shall find that we have no plausible room for admitting the absolute purity of the ice of the glaciers. A long sojourn amidst the ice of the Alps can alone reveal to us the true state of matters, and inform us of the imperfections and intimate defects of the glacier, for this ice, so exceedingly transparent, has its own share of impurities. In the year 1840, when we visited the grotto at the extremity of the upper glacier of Grindelwald, we discovered, not without surprise, a rolled pebble in the midst of the ice, and M. Agassiz directed the surrounding ice to be carefully removed, that, if possible, we might learn how it had been introduced. But we discovered absolutely nothing which gave us the slightest information on the point; the surrounding ice exhibited nothing particular; and as there was only this single pebble in the whole surrounding mass, we inferred it had probably fallen into a crevice which subsequently was closed up.*

M. Escher de la Linth was the first who shook our confidence on this point, by informing us he had seen in the glacier of Viesch large pebbles embedded in very pure ice. Nevertheless, we did not arrive at the knowledge of the true state of matters till the year 1842. In cutting a drain near the *Hotel des Neuchâtelois*, we noticed that one of the blue bands enclosed a regular bed of gravel nearly half an inch thick, and which penetrated into a depth of more than two feet. Our attention being excited by this occurrence, we examined the glacier with this object, and it was not long before we discovered gravel, and the minute debris of rocks in many other bands. At a later period, when we had learned to distinguish between the strata of the glacier and the blue bands, we regularly found that the greater number of the projections of the strata were accompanied with a slight bed of gravel; and, as it always happens, these traces of gravel become, in their turn, a distinctive character of the strata.

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* See Bibl. Universelle for April 1841, vol. xxxii. p. 359.

Whilst boring the gallery of infiltration, we likewise remarked traces of gravel in many places at the depth of eighteen feet. We even discovered in the roof of the gallery a common fly in perfect preservation, with its wings and legs entire, and at the distance of a few inches many fragments of different grasses equally well preserved. When we had once acquired the certainty that all parts of the glacier enclosed foreign bodies in greater or less quantity, wishing to take an approximate measure of this quantity, M. Agassiz caused the splinters of ice to be collected which the piercer detached from its point, which their lesser specific gravity brought to the surface from a depth of about 20 feet. These splinters were all of an ice apparently pure and perfectly transparent, especially where the air-bubbles did not superabound. The quantity of ice thus collected yielded 27 quarts (*litres*) of water. This water deposited, at the bottom of the containing vessel, which had been thoroughly cleaned, a layer of very fine siliceous sand, which weighed 64 grammes, so that each quart of water did not contain less than 2½ grammes of foreign matters, or about 50 grains.

What was the origin of this sand? A question this which may the more naturally be put, after it has been so long held and taught that the glacier ice does not contain, and could not contain, any foreign body. To explain the presence of this sand in the interior of a glacier, and at all depths, it is necessary to remember that each bed of snow which falls during the winter in the elevated regions remains uncovered during the ensuing summer,—that during this season the wind wafts towards it a certain quantity of dust and other foreign matters; for, although the dust is less abundant in these regions than in the valleys beneath, tempests are not wanting which elevate it from time to time. But the stratum of snow which is at the surface this year is covered the next by another, and the dust which had accumulated upon its surface, thus passes into the interior of the mass. At a later period, this snow is transformed into ice, and as the dust it contains is very fine, it will percolate, and probably circulate with the water, across the capillary fissures, so as to be uniformly spread throughout the mass. A direct proof that it is decidedly in this manner that these occurrences take place, is supplied by the fact, that the blue bands, in which water circulates much more frequently than in common ice, likewise contain a much more considerable quantity of gravel. This in no degree prevents a portion of the gravel from remaining on the surface of the beds, and hence their outcroppings are always more or less soiled.

We might cite, in confirmation of these observations, the fact, that the ice, however pure in appearance, never supplies a water that is perfectly limpid; it always, more or less, exhibits the milky hue which characterizes the water of all glaciers; and when it is left at rest, it usually deposits a residue which is somewhat flocculent. Chemical analyses of the water of glaciers have hitherto thrown no light on these points.

(To be continued.)

On the Composition of Avanturine-Glass. By Professor F. Wöhler.

Artificial Avanturine is a brown Glass with included small, very brilliant, spangles, which give it a peculiar shining appearance. It was formerly employed for various articles of art and ornament, and was manufactured at Murano, near Venice. When my friend and colleague Professor Hausmann (to whom

I am indebted for the historical information) visited the Venetian Glass Manufactories in 1819, it was no longer made, and nothing could be learned of its mode of preparation, which seemed to have remained a secret. The information on the subject, afforded by technological works, according to which avanturine-glass is produced by the fusion of fine leaflets of gold, copper, brass, mica, or talc, with glass, is incorrect, as is distinctly proved by microscopic examination.*

J. G. Gahn was the first who made the observation that the metallic spangles in avanturine-glass are *crystals*, which must have been developed during the cooling of the melted mass. When Hausmann was at Fahlun in 1807, Gahn shewed him these crystals under the microscope, and they then appeared to be regular six-sided and three-sided tables. The view of a piece of avanturine under the microscope, when only a moderate magnifying power is employed, is really surprisingly beautiful; and we perceive that each little spangle is a regularly formed brilliant crystal. The crystals are evidently segments of regular octahedrons, but so thin that we never can observe an entire octahedron. They are quite opaque; and the glass, in which they are embedded, is transparent when in thin plates, and has a yellowish colour, which in certain directions exhibits a tendency to bluish-green.

The formation of crystallised bodies, in masses which have been produced at a high temperature, is always deserving of attention, especially with reference to the formation of the crystallised compounds of the mineral kingdom; and hence it was of some interest to ascertain what the crystals in avanturine really are. The author of this paper, hoping that analysis would afford information on this matter, caused several specimens of avanturine-glass to be examined chemically by Mr Schnedermann. As the substance is not soluble in acids, the decomposition was effected by heat; partly along with carbonate of baryta, and in the other cases recourse was had to the usual method of analyzing the silicates. It resulted that pieces of avanturine, which were dissimilar in their external aspect, though they varied a little in the proportions of their constituent parts, still contained the same ingredients. The mean of several analyses afforded the following composition in the 100 parts :—

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* We must not confound with avanturine-glass the variety of quartz which, on account of its similar glittering property, has also been termed Avanturine.

Silicic acid with traces of tin,	65.2
Phosphoric acid,	1.5
Oxide of copper,	3.0
Oxide of iron,	6.5
Lime,	8.0
Magnesia,	4.5
Soda,	8.2
Potash,	2.1
Alumina and sulphuric acid, traces.	
	99.0

From this composition, we can only conclude that the artificial avanturine of Venice is an ordinary glass which owes its colour and shining appearance to copper, and copper probably in the form of protoxide. However, the perfect opacity of the extremely thin crystalline leaflets was against the last idea. The microscopic examination of the fine powder of metallic copper, which was obtained by reduction with phosphoric or sulphuric acid from the solution of a salt of copper, afforded decided evidence on the point. Such copper-powder, examined under a magnifying power of 50 or 80 times, presents precisely the same appearance as the spangles in the avanturine; it consists entirely of brilliant octahedral crystals which exhibit sometimes three-sided and sometimes six-sided faces. Hence it cannot be doubted, that the crystals in avanturine-glass consist of metallic copper, which has been separated from the melted glass containing oxide of copper, by means of the addition of some reducing substance. There are two other circumstances in favour of this view: first, that this glass is so easily fusible that it becomes liquid much under the fusing point of copper; and, secondly, that Hausmann possesses a copper slag from Biber in Hesse, which includes octahedral spangles, precisely similar to those of avanturine.*

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* Götẗing. Gelehrt. Anzeig., No. 179 and 180; and Poggendorff's Annalen, 1843, No. 2, p. 286.

On the Forms assumed by Granite and Gneiss at the surface of the Earth. By Baron LEOPOLD VON BUCH.

IN almost every locality where granite occurs, it is quite apparent that the projecting mass form part of an ellipsoid with a convex surface. This is beautifully seen in the Brocken, when we ascend from Elbingerode by Schierke. These ellipsoids are of greater or smaller extent, sometimes many German miles in size, as in the Riesengebirge, in the Bohemian and Moravian mountains, in the Odenwald, in the Black Forest, and in Cornwall; or only the size of hills, but then they are grouped together in larger numbers, as in the southern part of Hindostan, or in Sweden and Finland. If the granite is covered by gneiss, then the latter follows the form prescribed for it by the granite. In the interior, these convex masses are formed of layers, which lie concentrically over one another, always becoming smaller and smaller, till they form a kind of cylinder of but small breadth. The position of the superimposed mountain rocks, and the change produced at their boundaries by the granite, lead to the very probable conjecture that the granite itself has been raised from the interior like a kind of bladder, and that the rocks covering it have been either pushed aside, or completely converted into new rocks. The division into layers was a consequence of the cooling of the granite which was raised at a high temperature; for, the experiments of Gregory Watt, and Gustav Bischoff of Bonn, have directly proved such a laminated separation of cooling masses. The surface of this convex mass of granite is often covered with an inconceivable number of blocks that are not far distant from their parent rocks, which, however, often rise to great heights. So it is on the Brocken, on the Achtermanshöhe, on the Riesengebirge, at many localities in the Black Forest, and almost at every place where the granite is of some extent. These desolate block-covered surfaces have given rise to the legend of "Devil's Mills" (*Teufels Mühlen*); they are also sometimes termed "Rocky seas" (*Felsen Meere*), and, in Greece, "Devil's floors" (Ulrich's Reise, vol. i., p. 121). They also are *results of the contraction*, and consequent *separation* of the cooling surfaces; and hence it is conceivable how granite should be more covered with such blocks than other mountain-rocks. The layers are often smooth on their surfaces, just as if they had been polished. An observation which can be made in the middle of the town of Stockholm proves that these are produced by rubbing on one another, that they are true friction surfaces. Proceeding from Södermalm's sluice, "Stora Glasbruksgata," to the Catharine Church, we find convex strata of gneiss, which are transversed by many small granite veins. These veins, however, regularly exhibit a shift as they pass from one layer to another, so that it is evident how one layer has advanced over the one immediately under it, and undoubtedly not without smoothing and polishing itself on the rubbing surface. The under layers, also, which are covered by others, are just as smooth and as polished as the outer one at the surface; and, hence, every *external cause* of the smoothing, such as the movement of masses of ice or of blocks over the surface, is completely excluded, and must be rejected.

The whole of Finland, and the greater part of Sweden, are covered by such small granite and gneiss systems, composed of polished layers, and the mode of distribution is very distinctly and beautifully seen in the ideal section which accompanies Engelhardt's Sketches of Finland. This display terminates at the south coast of Finland, and we find on the other side of the gulph, in Esthland and Liefland, indications of extraordinary tranquillity in the mountain rocks—a tranquillity and an uniformity which prevail over the greater part of European Russia, and cannot be equalled in the whole of the rest of Europe. The Silurian strata in Esthonia not only lie very regularly and quite horizontally on one another, but they are likewise so little altered, that the organic remains which they contain are almost every where easily recognised, and can with facility be removed from the matrix. The newer rocks follow in large curved masses as far as the Ural and the granite ellipsoid of the Ukraine.

That the gneiss which covers the ellipsoids of granite in Sweden and Finland, like all gneiss generally, owes its origin to *metamorphism*, which has formed it from previously existing slates at the time of the elevation of the granite (by the penetration of felspar between the slates, and the conversion of the slaty mass into mica), is a view which has been adopted by the most eminent geologists for many years, and which has latterly been not a little confirmed by many acute observations and reasonings of Messrs Dufrenoy and Elie de Beaumont in the illustrations of the geognostical map of France. According to this view, all the gneiss of Sweden and Finland involves the supposition of the previous existence of Silurian strata over the whole north of Europe; for, where unaltered strata present themselves in that region, they belong to the oldest strata of the transition series. The action of this immense metamorphism terminates with the Gulf of Finland, and it does not appear again in Russia.

Every map of these northern countries shows quite distinctly that the Gulf of Finland is a continuation, having the same direction, and placed in the same latitude, of the strait which separates Norway from Jütland; and, exactly in the same direction and latitude likewise, Sweden is traversed by a hollow in which a great series of lakes succeed one another, by means of which it becomes possible to convey ships of war through the solid land from the North Sea to Stockholm, without having recourse to the Baltic. It is only in this hollow that the unaltered transition strata present themselves, up the Motalelv and in the West Gothland plains, and the strata contain the same organic remains which occur near Petersburg and Reval, and hence evidently belong to the Silurian series.

It is not impossible that the remarkable mountains of West Gothland, viz. *Billingen* with its prolongations, the *Kinnekulle*, and the *Hallberg* and *Hunneberg*, near *Wenersborg*, may one day give us the key to the cause of these inlets being the boundaries of the action of the granite and of the metamorphism of the slates into gneiss. The hills of which I have spoken rise like castles above the plains, and are the only ones whose *steep acclivities* are composed of unaltered fossiliferous strata of the transition series. At a little distance from their bases, such strata are no longer met with in the flat country. Each hill is also covered by a mass, often of considerable dimensions, of a rock which is probably augitic, and is black and granular, like the basalts of Staffa and the other Hebrides. Now, as observations made in Germany and Scotland have fully proved that such augitic rocks proceed from the interior in irregular masses and veins, and have spread themselves over the surface of the strata that have been broken through, it cannot be doubted, that each one of the West Gothland hills also contains a basaltic cylinder, irregular mass, or vein, which unites the upper bed with a basaltic or augitic mass that extends *far beneath* the granite. The hill of Billingen is quite similar to the Meisner in Hesse, in which numerous shafts, leading from the outer circumference to the interior, have brought to light the internal basaltic nucleus. The gneiss every where surrounds these hills like a projecting rampart, *but is never in immediate contact with them*, and it is, in fact, very much to be doubted, if in the whole of Skaraborgslän there is a single point where gneiss or granite forms the underlying mass of the hills composed of upraised transition strata.

It is, therefore, the basaltic mass concealed in the interior which *has protected* the silurian strata that were penetrated and raised by it, and which *has withdrawn them from the metamorphic action of the granite*, and the other matters that accompanied it at its protrusion. A little further (at the Hunneberg, near Floh-Kyrcka, about a German mile distant), the basaltic rock terminates, and the granite again presents itself at the surface, at least it does so in Smoland as far as Schonen, but not in Esthland and Liefland.

It is with some surprise that we again meet with the convex and smooth layers of granite in *Switzerland.* One would not have anticipated this occurrence in a chain where there is so much disturbance, and in which the mountains rise into such bold peaks and ridges. They are, however, no longer observable on the summits, but in the valleys they are exhibited of large dimensions and of wonderful extent. There is a fine example in the well known Höllenplatte, above Handeck, on the Grimsel, which is represented in Agassiz' work as an illustration of glacier action. Saussure, on the other hand (iii. p. 459), saw there *strata,* one above the other, "*convexes, posées en retraite les unes sur les autres, comme d'immenses gra-*

dins;" and this view seems to hold good with regard to the whole of the pass of the Grimsel. Near the wooden bridge which, above Handeck, leads from the left to the right bank of the Aar, we see quite at hand smooth strata which become concealed under those that are superimposed, and which run under the latter with the same smooth surfaces. Beautiful convex masses, forming layers the one above the other, again appear on the Sidelhorn acclivity of the valley of the Grimsel, and on the pass itself. Saussure would scarcely have recognised a polish produced by glaciers in the "*Rochers moutonnés*," which are formed by these layers; in fact, the appearance seems to indicate and to prove a much more comprehensive and more widely extending cause than the action of glaciers could have been.*

The Cells in the Ovum compared with Corpuscles of the Blood.—On the difference in Size of the Blood-corpuscles in different Animals. By MARTIN BARRY, *M.D., F.R.S.S.L. and E.*†

1. In several communications presented to the Royal Society, and printed in the Philosophical Transactions, it has been my endeavour to shew that the remarkable process effecting the division and subdivision of what is usually termed the "yelk" in the mammiferous ovum, is to be recognised in other cells; and nowhere more distinctly than in certain states of the corpuscles of the blood. In proof of this, I gave the delineations, figs. 1 to 5, along with many others. Figs. 1, 2, 3, represent blood-corpuscles (cells) of the Sparrow. 1. The nucleus single: 2. The nucleus dividing into two parts: 3. This division is complete. Figs. 4, 5, are blood-corpuscles (cells) of the fœtal Ox, three-quarters of an inch in length: 4. The nucleus consists of two discs: 5. The discs have separated and much increased in size, and they are passing into the state of cells.

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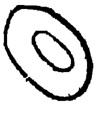

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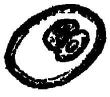

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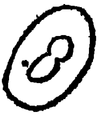

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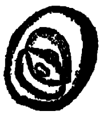

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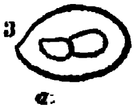

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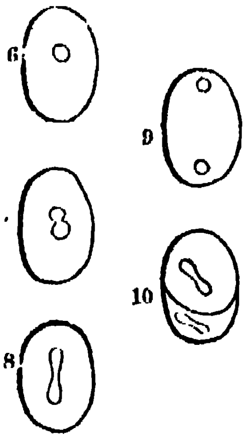
* Monthly Reports of the Academy of Sciences of Berlin, and Poggendorff's Annalen, 1843, No. 2, p. 289.

† London, Edinburgh, and Dublin Phil. Mag. 1st June 1843.

2. Such having been my views, it was very satisfactory to meet with the following confirmation of them. In a lecture delivered a few weeks since at the College of Surgeons, Professor Owen exhibited the sketches 6 to 10, in a thesis by Dr Bagge,[*] representing successive stages in the development of the ovum of an intestinal worm; and stated the results of Dr Bagge's observations. I find the following remarks on this subject in the published lecture of Professor Owen:—"There is a close and interesting analogy between the above phænomena [observed by Dr Bagge, fig. 6 to 10] which were published in 1841, and some of those communicated by Dr M. Barry to the Royal Society, in January 1841, and published in the Philosophical Transactions of the same year. The clear central nucleus of the blood-corpuscle is there shewn to form two discs,[†] which give origin to two cells. We may, likewise, discern in the pellucid nucleus of the yolk, dividing and giving origin to two yolk-cells, according to the German author, the hyaline nucleus of Dr M. Barry."[‡]



3. Professor Rudolph Wagner observed that the size of the blood-corpuscles in the naked Amphibia is "so much the larger, the longer the gills continue in the larval state." Thus, the blood-corpuscles are larger in the Newt than in the Frog. He hence conjectured that the Proteus and Siren, because they permanently have both gills and lungs,—being therefore permanently larvæ,—would be found to have the largest blood-corpuscles. In the Proteus, he had the opportunity of seeing the idea realized.§—This connexion between the size of the blood-corpuscles and a larval condition of the animal, I believe has not been explained.

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[*] *De evolutione Strongyli auricularis et Ascaridis acuminatæ viviparorum.* Erlangæ, 1841.

[†] See Phil. Trans. 1841, pl. 18, fig. 37, [and pl. 17, fig. 24.]

[‡] Hunterian Lectures, by Professor Owen, F.R.S., from Notes taken by W. W. Cooper, M.R.C.S., 1843, No. 3, p. 78.

§ See Proceedings of the Zoological Society, Nov. 14, 1837.

4. On first seeing the large cells in the mammiferous ovum,[*] I was struck with the resemblance they bore to the corpuscles or cells of the blood in, for instance, the Batrachia; which was also remarked by Dr Roget on seeing my delineations of the former: and I have since (§ 1, 2) shewn them to be perpetuated by the same means. Finding also in the blood of the mammiferous *embryo* corpuscles or cells (figs. 4, 5) like the ordinary blood-corpuscles or cells of the *adult* Batrachia, &c., I conceived that the difference between the condition of the blood corpuscles in the embryo and in the adult of the same animal, was referable to a difference in the degree of their development as cells.[†]

5. Now, there are facts, I think, which leave little doubt that the blood-corpuscles—not only in the embryo, but at all periods of life—are descended from the two cells constituting the foundation of the new being in the ovum; cells arising out of previously existing cells, by self-division of the nuclei.

6. When tracing the early stages in the formation of the embryo, I shewed that, as the cells thus increase in number, they diminish in their size. Have we any proof that this diminution in size ceases in later stages? Is it not rather to be presumed that it continues? and, indeed, does not the difference in size between the corpuscles of adult and fœtal blood render it probable that this progressive diminution in size goes on? If so, the younger the larva is, the larger may be its blood-cells. And as a larval state in the Batrachia, &c., is indicated by a retention of the gills, is it surprising that we find their blood-corpuscles large in proportion to the length of time during which they retain the gills?

7. I cannot doubt that a law of the kind now mentioned—progressive diminution in the size of cells—is *general* in its operation; and if so, it may regulate the magnitude of the corpuscles in other blood.

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[*] Researches in Embryology, Second Series. Phil. Trans. 1839, pl. 16, fig. 150½, &c.

[†] On the Corpuscles of the Blood, Part II Phil. Trans. 1841, p. 206.

On the Comparative Level of Lakes and Seas, as the Caspian, Lake Aral, Black Sea, Red Sea, Mediterranean, Lake Tiberias, Dead Sea, Atlantic, Pacific, &c. BY BARON ALEXANDER VON HUMBOLDT.

'The Caspian Sea has presented two problems which have long been subjects of discussion,—the problem of its isolation, and that of the relative height of its mean level; and there has been a similar fluctuation on both these points. Inasmuch as, by the assiduous labours of Messrs G. Fuss, Sabler, and Sawitsch, the height of the waters of the Caspian was perfectly ascertained in the year 1837, we might pass in silence the attempts which were made by means of barometrical measurements. I wish, however, to record here some of the results which, for 60 or 70 years, most engaged public attention.

*Chappe** found from the barometrical observations made by Dr Lecre at Astrakhan, from the years 1732 to 1749, that the Caspian was depressed 51 toises. This result he designated as "evidently absurd."

Inochodzow, in comparing the mean barometric heights of Kamychine, a small town in the government of Saratov (means deduced from observations from October 1770 to August 1774), with the corresponding barometric means of St Petersburg, concluded from this comparison, that Kamychine (lat 50° 5′), upon the banks of the Volga, is 171 French feet, (28.5 toises), below the level of the surface at St Petersburgh.† There is a difference of 4° of latitude between Kamychine and the mouth of the Volga; and there is 10° between Kamychine and St Petersburgh. It is very likely that a remark respecting the depression of the basin of the Caspian, which is to be found in the celebrated work of the Abbé Raynal upon the colonies, is grounded upon his cognizance of the barometric calculations of Inochodzow. This eminent Russian philosopher mentions that, previous to his time, *Chretien Mayer*, in a memoir regarding the transit of Venus (p. 316), had valued the negative height of the Caspian at 101 feet (17 toises), grounding his calculation upon the barometrical observations of the traveller *Lerche*. It is almost superfluous to point out, that this coincidence, with the first provisional result derived from the grand trigonometric levelling of 1837, is altogether accidental. In fact, it is in hypsometry as in the determination of different astronomical positions. When a point has for a long time oscillated in our charts from north to south, we always find some one or other ancient chart which indicates the true latitude, in agreement with the result of the latest and most difficult operations. Pallas‡ preferred a valuation which differed but little from that of Mayer. He assigns 10 toises for the difference of the level of the waters of the Volga and the Don, at the place where the rivers most approximate. When to this we add the 8 toises which M. Goebel found Zaritzin higher than the level of the Caspian, by a very accurate barometrical levelling, we have for this level —18 toises. Nevertheless, we must remember that, according to a geometric levelling recently made by the officers of the Imperial Corps of Roads and Highways, the differences of the level of the Don and Volga, between Katchalinsk and Zaritzin, is twice as much as that stated above, or 21.2 toises. MM. Parrot and Behagel have found it barometrically to be 27 toises. From my own observation (on the 21st October 1829), I found a difference somewhat less between the Volga and Tchivskaya upon the Don.[*]

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* Voy. en Sibérie, T. ii. pp. 487-491.

† Nov. Acta Acad. Petrop. T. xii. p. 506, 1801.

‡ Reise, T. iii. p. 316.

The barometrical means which the *Acta Acad. Petrop,* 1782, p. 24, supply for Astrakhan, Irkoutsk, Moscow, and St Petersburg, make the Caspian —45 toises.

The astronomer *Wisniewski,* from barometric observations of 3 years' continuance, fixed the level at —257 feet, or 42.8 toises. It is not, however, probable, that it was to this conclusion that Thomas Young refers,[†] where he affirms that the depression of the Caspian appears to be nearly 300 English feet (—47 toises).

The barometric levelling by stations, executed in the year 1811, by *MM. Parrot* and *Engelhardt,* between the Caspian and the Black Sea, gave in going and returning, in 27 days, —54.2 toises; in 30 days, —47.1 toises; mean, 50.6 toises.

Monteith believed that he ascertained by the determination of the boiling point of water, that the level was —61 toises; and *Loktin,* from barometrical observations from 1805 to 1811, made at Astrakhan, that it was —39 toises.[‡]

MM. Hofmann and *Helmersen* conducted with much care, in 1825, a barometric levelling by stations from Orenbourg to Gourief, situated at the embouchure of the Jaïk (Oural) into the Caspian Sea. They found by this levelling, and from corresponding barometric observations at the extreme points, that Orenbourg was elevated 52 toises above the level of the Caspian Sea. Now, M. Galle assigns from the whole barometric observations of MM. Hofmann and Karelin at Orenbourg, that this fortress is 39 toises above the ocean. From this it may be deduced, that the Caspian has a depression of —13 toises, a result which is remarkably accurate, but which was for long misunderstood, because it was supposed that Orenbourg stood but little above the level of the ocean. The barometric observations which were made together by MM. Hofmann, Helmersen, Rose, and myself, from the 12th to the 21st of October 1829 on the shores of the Caspian, were compared with corresponding observations at Kasan; and they have made us sceptical ever since respecting any very great depression of the Caspian.

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[*] Parrot, Reise zum Ararat, 1834, p. 13, 192.

[†] Course of Lectures, 1807, vol. i. p. 571; vol. ii. p. 367.

[‡] Pansner Höhen im Europ. et Asiat. Russland, 1836, p. 23.

M. Parrot, whose premature loss science has to regret, during his memorable journey to Ararat, conceived doubts concerning the accuracy of two barometric levellings by stations which he had executed with extreme care, though labouring under fever, in the year 1811. In 1830, he had the energy to repeat this painful labour between Astrakhan and New Tcherkask, which occupied him for twelve days. From these observations, he concluded, that there exists only a slight difference in the level of the surfaces of the water of the Black Sea and the Caspian.* It would not be easy sufficiently to commend the stern and noble candour which M. Parrot has displayed in discussing his own barometrical observations. The source of error ought to be sought for alone in the barometric method of levelling by stations, to which the mode of observations made at the extremities of a geodæsical line is greatly preferable. In the partial levelling by stations, the errors accumulate by the influence of the frequent change of local temperature, and by the comparison of two instruments, the one of which, and the same one, always preceding the other. Another barometric levelling by stations was likewise executed in the years 1838 and 1839, simultaneously with the geodœsical levelling by M. Fuss. By various accidental causes it again yielded a result of —47 toises, a depression almost identical with that given by the first operation by stations, in 1811, by M. Parrot.

The results derived from the barometric observations taken on the shores of the Caspian and Black Seas have been very different. M. Sawitsch, from observations made at Taganrog and at Astrakhan, has made the depression — 22.2 toises; M. Göbel† from observations at Astrakhan and Sympheropol, has made it 15.9 toises; and M. Lenz, by comparing 3510 barometric observations made in 1830 at Bakou by M. Meyer, and at Taganrog by M. Meyer, 16.8 toises.‡

If, thanks to the admirable labours of MM. Fuss, Sablor, and Sawitsch, we now know with mathematical precision the real depression of the level of the Caspian Sea, amounting to 12.7 toises, the accounts of the height between the Caspian and the Lake Aral are not equally certain. These accounts have been taken only by means of *barometric levelling by stations* conducted during the military expedition of l'Oust-Ourt,§ during the winter of 1826. After the troublesome and unsatisfactory experiment of comparing the goodæsical levelling with the barometrical of the year 1837, it is impossible to have much confidence in the barometric method employed.* As the Lake Aral has been found to be only 34 feet (5.6 toises) above the level of the Black Sea, and that in the two barometric levellings by stations in 1811 and 1837, the error exceeded 224 feet (37.3 toises), it may well be that the Lake Aral may be at the level of the waters of the Caspian, and that all the Aralo-Caspian basin has a depression of 76 feet (—12.7 toises). But without adopting this conclusion, grounded upon the analogy of the barometrical operations of 1811, 1826, and 1837, and excluding also the whole shores of Lake Aral, we still find, as the result of our actual knowledge, an extent of *continental land* of more than 8000 square marine leagues depressed below the surface of the Black Sea. The geodæsical line of *zero height*, that is to say, that which unites the points of the surface placed on the level of the Black Sea, traverses the Volga between Zaritzin and Saratov. Now, from the extremity of the delta of the Volga to Zaritzin, there are 3° 40′ of latitude—a distance equal to that from Paris to Grenoble, or equal to four-fifths of the extent of Spain. To the east of Zaritzin, the shores of Lake Elton† and Kalmykova, upon the river Jaïk, are from 5 to 12 toises below the level of the Black Sea. It is also believed that the country which encloses, to the west of Kalmykova, the salt-lakes of Kamych-Samara, have a depression of —23 toises, consequently being ten toises beneath the level of the Caspian Sea.‡ M. de Struve remarks, " that the line of the *zero* level surrounds a submarine space (placed beneath the surface of the Black Sea) greater even than the surface of the Caspian; and that in the geodæsical levelling of 1836 and 1837, the limits of this *vast Asiatic depression* towards the west, was at a distance of 12 marine leagues from shore.§ We have seen that to the north‖ and north-east, the depression extends above 70 leagues. Limiting ourselves, then, to an estimate of 8000 square marine leagues less than that of M. de Struve, we obtain for the *area* of the total depression,—including that which is actually covered with the waters of the Caspian Sea,

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* See *Reise nach dem Ararat*, T. ii. p. 12–31; likewise Letter of M. de Humboldt, of date 28th May 1834, stating doubts of a depression of 200 or 300 feet attributed to the basin of the Caspian Sea. T. ii., p. 191–198.

† M. Göbel has compared the barometric observations of M. Offe at Astrakhan, those of M. Steven at Sympheropol, and his own at the Sea of Azov. Göbel, Reise, T. ii. p. 193.

‡ Recueil des Actes de l'Acad. de St Petersburg, 1836, p. 20.

§ See T. i. p. 421. The direct result obtained by MM. Sagoskin, Anjou, and

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Duhamel, was, that the level of the waters of Lake Aral was 117.6 English feet (18.3 toises) above the level of the waters of the Caspian.

* See an interesting Memoir of M. Lenz: *Considerations Mathematiques sur les nivellements par Stations an moyen du barometre.* (Bull. de St Petersburg, T. i. p. 51 and 63.

† I have found the south-west extremity of this lake generally very erroneously placed in our maps in lat. 49° 7′ 17″, and long. 44° 15′ 36″.

‡ Göbel, *Reise*, T. ii. p. 200.

§ Bull. de l'Acad. de St Petersb., T. iii. p. 368.

‖ It results from the barometric observations of M. Göbel, considering the zero at the level of the Black Sea, and admitting —12.7 toises for the Caspian Sea; that the Volga, near to Saratov, is at an elevation of + 6 toises; Zaritzin is —5 toises; the Steppe Khotchetaevka —12 t.; the Lake Bogdo —3 t.; the Lake Arsangar + 12 t.; and the Mount Grand-Bogdo + 87 t. According to MM. Parrot and always excluding Lake Aral,—more than 18,000 square marine leagues; in other words, a surface of 900 square leagues larger than that of France. It is earnestly to be wished that the Imperial Academy of St Petersburg will continue the beautiful geodæsical survey commenced under its auspices, and that that illustrious society will not only ascertain the true difference of the heights of the Aral and the Caspian, but that, so far as these regions are accessible to such peaceful occupations, they will also succeed in procuring the *zero geodæsical line* between he Volga and the Iaïk, between the Iaïk, the Emba, and the north-east extremity of the Aral Sea, and finally, between this sea and the Aksakal-Barbi.*

M. Arago who, in his writings, always ascends with success to first principles concerning the physical constitution of the globe, has, in his memoir upon comets, pointed out a curious work of Halley,† in which the English astronomer, as early as December 1694, mentioned the *great depression* of the Caspian, which he attributes to the shock of a ball of immense dimensions; in other words, of a comet. "The vast depression," observes M. Arago, "of a whole country, in former times appeared of an explanation too difficult for the action of any ordinary forces; and in despair of such causes, recourse was had, as in many other circumstances, to an action proceeding from the celestial spaces. In the present state of our geological knowledge, this idea of Halley will not receive much favour. Scarcely any one now doubts that the isolated peaks, and the longest and most elevated ranges of mountains, have proceeded from the interior of the globe by the way of upheaving. But does not this very upheaving imply the production of a void beneath the surrounding districts, and the possibility of this subsequent subsidence? When we cast our eyes upon a geographical chart, we easily perceive that no part of the world exhibits so many upraised masses as Asia. Around the Caspian Sea, at greater or less distances, we find the high lands of Iran and of Central Asia, the mountain chains of Kouen-lun, and of Hindoo-Kho, the mountains of

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and Behaghel, the Volga near Zaritzin is 27 toises lower than the Don 72 versts above the Patiisbansk. I assign these valuations, remembering how uncertain a barometric levelling is in the plains and for the measurement of small heights, when the differences in the height of the columns of mercury do not attain to 2 millimetres, and are grounded upon a small number of observations.

[*] I attach great importance to the determination of the height of the land in the Steppe, which the ancient hollow, running S.S.E. and N.N.E., and of which I have often spoken, follows. I find in my correspondence with M. St Martin, that this *savant* had heard of "a tradition of the Avares, according to which this people allege that they had quitted their original place of abode (at the foot of Altaï?) on account of the sudden drying up of an interior sea, and of an increased dryness of the Steppe." I am quite ignorant of the source whence M. St Martin procured this notice, which we might be tempted to associate with the Chinese story of the disappearance of the " Sea of bitter waters."

[†] *Some considerations about the cause of the Universal Deluge*, Phil. Trans, vol. xxxiii. p. 122. " A shock of a comet may have occasioned that *vast depression* of the Caspian Sea and other great lakes in the world."

Armenia, and those of the Caucasus. Hence, without having recourse to a comet, is it not natural to suppose that the upraising of the enormous masses of rock we have just named, was sufficient to induce the sensible depression of the intermediate localities? This solution of a curious problem in physical geography which the shores of the Caspian has originated, may so much the less give rise to serious difficulties, inasmuch as in those very regions the surface, even at the present day, is not altogether stable; and the bottom of the Caspian Sea itself presents alterations of elevations and depressions. It is, however, true, that the fact under discussion will lose much of its interest, if we investigate it as a simple *meteorological phenomenon.* Suppose that a Julia island should happen to rise in the middle of the Straits of Gibraltar, and close up the entrance. Immediately, the rapid current which a portion of the waters of the ocean constantly pours into the Mediterranean would cease; immediately the level of the Mediterranean would fall, for the total volume of the rivers it receives would not compensate, as it appears, for the loss resulting from evaporation. During this gradual lowering of the level of the sea, parts which are now submerged would appear above the waves, would connect themselves with the neighbouring continents, and would remain, as at present, beneath the level of the ocean. This, perhaps, is the solution of the whole problem of the Caspian, and especially, if with some geologists we add, that, in this sea, large volcanic crevices, from time to time, permit its waters to dissipate themselves in the bowels of the earth, and thus render the difference more sensible which, even without this occurrence, already existed between the effects of the annual evaporation, and the waters supplied by the Volga, the Oural, the Terek, and other rivers."*

These judicious considerations acquire still greater importance when we consider the additional information collected since the days of the illustrious Pallas,† especially from the maps of Major Khatov,‡ and from the detailed investigations of M. Parrot, upon the plains which extend in the steppe of the Kalmucs and Turcomans, between the Black and Caspian Seas, in the 45° and 47° of latitude. A slightly elevated ridge detaches itself from the Caucasus, running from the Elbrouz towards Stavropol, in the direction from south to north. Upon the opposite slopes of this ridge there take their origin, first, the Kouban and the Terek,§ and then the Iegorlik and the Kouma, which, at the end of summer, sometimes lose themselves in a lake of the Steppe, without ever reaching the Caspian Sea. Still farther on, at the northern extremity of the ridge, the bifurcated Kalaüs takes its rise.* This last river, whose course is very variable, commingles its waters with those of the Manetch, an affluent of the Don. It is even regarded as the principal source of the western Manetch. Another branch of the Kalaüs proceeds to the eastern Manetch, and spreads out near to Gouidouc, a post-station on the road from Astrakhan to Tiflis, into a large lake which the Kalmucs call by the name of Kokoussoun. The conformation of the district, which gently undulates along the Manetch, and to the north of the Kouma, is very remarkable. The course of the Manetch or Manytch is 500 versts in length; its fall is so trifling that, according to the testimony of General Bogdanovitch, who has examined its windings, its waters during the summer actually follow the direction of the wind. The river at the present time does not throughout the upper course approximate nearer than 70 versts to the weedy shores of the Caspian Sea; and yet, without doubt, it was in this part of the Kalmuc Steppe, we must suppose, that, previous to those times we designate historical, a communication existed between the basins of the Caspian and Black Seas.

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* See *Notices Scientifiques* by M. Arago, inserted in the *Annuaire du Bureau des Longitudes*, 1832, p. 352.

† Pallas, *Voyage dans les Provinces méridionales de la Russie* in 1793 and 1794, T. i. p. 235. Dureau de La Malle, *Geographie physique de la Mer-Noire*, p. 176, 194, and 264. Compare also Pansner and Zeune, in the Journal of M. Berghaus, 1836, No. 140, p. 179 and 187.

‡ Maps (in 10 sheets), published by *l'Etat-Major* of the Imperial Army, representing the country between the Black Sea and the Caspian.

§ Since the commencement of the 17th century, great changes have taken place

The work in which MM. Fuss, Sawitsch, and Sabler are about to describe the district which has been the theatre of their very careful geodæsical labours, will shed new light on those views, several of which may appear to be rash. I shall here limit myself to the statement of a very curious fact which, according to the report of some of the natives who seem worthy of credit, was received by M. Parrot at Gouidouc, namely, that there existed, in times quite modern, a communication between the eastern Manetch and the Caspian Sea. All these facts, however, render it highly probable, that, in ancient times, previous to the time when the sand embankments and the downs, accumulated by the winds, had changed the surface of the soil; and, previous to the sea of Asov being restricted to its present limits, a strait or natural canal had conducted the waters of

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at the embouchure of the Terek and the Tumen, near which was situated the fortress of Terki, which was constructed under the Czars Michel-Feodorovitch and Alexi-Michailovitch, and destroyed by the orders of Peter the Great in the year 1722. The site of this fortress has since been entirely covered by the Caspian Sea; a phenomenon the more extraordinary, that at the same epoch the waters of this sea apparently sank at the custom-house of Astrakhan, and towards the mouths of the Volga. See the Memoir of M. Hamel, *Sur un expedition minéralogique au Caucase, faite in 1628, sur les auspices du Czar Michel Feodorovitch.*

* Parrot, *Reise zum Ararat*, T. ii. p. 12-25, and p. 33-36. The waters of the Kouban, the Iegorlik, and the eastern Manetch, flow to the Black Sea; those of the Terek and of the Kouma to the Caspian Sea. The Kalaüs forms, by its bifurcation, to the west, the western Manetch, which dilates into a salt lake (the lake Manetch), and receives the Iegorlik before it reaches the Don. The eastern branch of the Kalaüs, which merits a more special examination, seems to form the eastern Manetch. The river Kouma does not in all seasons reach as far as the Caspian, often terminating in summer in a fresh-water lake, the lake of Iarligor.

the Euxine to the Caspian Sea.* This strait must have produced a marine current from west to east, similar, on a small scale, to that which issues by the pillars of Hercules into the Mediterranean, and which may be traced even to the coast of Pelusium, where it opposes the too rapid increase of the Delta of Egypt.†

Without having recourse to the oscillations which, at the epoch of great geological revolutions, have made the recently solidified crust of our planet often rise and fall in the plains, we may suppose that, even still, many continental regions, supported by solid rocks, are found at a level inferior to that of the ocean, but that, from the large accumulation of abraded soil, superimposed upon the tertiary and secondary rocks beneath, the ancient difference of level is hid from our observation. Such a difference, notwithstanding, remains very visible in many parts of the coast of Holland, and in the north-east of Germany; as it is also in Egypt in the *Natron lakes*, visited by General Andreossy; as also in the *bitter lakes* of the isthmus of Suez, when they are left dry,‡ or contain but an inconsiderable depth of water. The difference of the level of the surface of the seas, rated at five toises at the isthmus of Suez by M. Le Père, and of half a toise at the isthmus of Panama, by MM. Lloyd and Falmarc, are phenomena of an entirely different kind from that with which we are now engaged. They are the effect of currents, of the preponderating direction of certain winds, of the height of the tides reflected by the windings of the coasts, of the form of the canal§ by which they flow or ebb, and finally, of the variations of the density of our planet.

It is the isolated basin of the Jordan and the Dead Sea, whose hypsometric relations have so recently engaged the attention of travellers, which offers the closest analogy with the isolated basin of the Caspian Sea. The determination of the boiling point of water, which, to be exact, requires much care, and barometrical measurements, which, unfortunately, did not accord, at first gave for the depression of the level of the Dead Sea beneath the level of the Mediterranean results which varied between 500 and 1100 French feet (between 83 and 183 toises). The barometer of M. Schubert, and the barometer of Messrs Moore and Beck, indicated almost at the same time, in the month of April 1837, the existence of an enormous hollow.* The two English travellers estimated the depression of the Dead Sea at — 93 toises. M. le Comte de Bertou has the merit of having first determined the amount of the depression by barometrical measurements. M. Caillier,† after discussing a portion of the measurements, concluded it amounted to — 208 toises (— 406 metres). More recently,‡ M. de Bertou, by combining, in his memoir, all the observations made in March 1838, and in May 1839, estimated it at — 215 toises (— 419 metres). M. Schubert calculated the level of the Sea of Tiberias at — 89 toises (— 535 feet), but he did not measure the Dead Sea. M. Russegger, whose journies in Africa and Asia have enriched geology with many important observations, has watched the indications of the barometer in November and December 1838, during 15 days at Jaffa, at Jerusalem, and at the Dead Sea. He thinks it possible, owing to the want of corresponding observations, that the result of his measurements may leave a mean doubt of 200 feet at most; but, allowing for this doubt, he calculates that *higher* than the level of the Mediterranean,§ Jerusalem stands + 2479 feet (413 toises), and Bethlehem + 2538 feet (423 toises); and *below* the level of the Mediterranean, the Lake Tiberias is — 625 feet (— 104 toises), and the Dead Sea — 1341 feet (— 223 toises, or — 435 metres). In proportion as, in the geodœsical labours, from the variations of successive results, the level of the Caspian has *ascended* from — 50 to — 13 toises in relation to the Mediterranean, the level of the Dead Sea has proportionally *descended*. After the numerous doubts which have prevailed regarding the great depression of this last level, a trigonometrical survey, executed in the autumn of 1841 by Lieutenant Symond of the British Royal Navy, has given the result, that the surface of the waters of the Dead Sea is 251 toises (489 metres) lower than the highest house of Jaffa, and probably 219 toises (427 metres) lower than the surface of the Mediterranean. This trigonometrical result differs accidentally only 8 metres from the result of the barometrical measures of MM. Bertou and Russegger. The geological problem of the depression of the valley of the Jordan and of the Dead Sea is all the more important, that it is intimately connected, I will not say with the destruction of the five cities of the plain, but with the impossibility, long ago established by M. Letronne, of the non-communication of the Jordan, in historic times, with the Elanitic gulf of the Red Sea.

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* The mean level of lake Manetch, the enlargement of the western Manetch, and consequently also of the western branch of the Kalaüs, does not appear to be elevated above 3 or 4 toises above the level of the Black Sea.

† Letronne, *Mém. sur l'isthme de Suez, et le canal de jonction des deux mers* (Rev. des deux mondes, July 1841.)

‡ In this state, the *Bitter lakes* are 20 feet beneath the level of the Mediterranean.

§ M. Arago, in discussing the height to which the level of water maintains itself in a gulf which communicates with the ocean by a narrow canal, has pointed out that it is not mathematically proved, that, by a canal of a certain form, the quantity that enters and escapes will be the same. An accumulation of water, or a rise in the level of a gulf or a narrow sea, may be produced by this cause alone.

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* *Journal of the Roy. Geog. Soc.* vol. viii., p. 250. *Jameson's Edin. Phil. Journ.* vol. xxix. p. 96. "Professor Schubert of Munich, two Englishmen Messrs Moore and Beck, and M. J. de Bertou a Frenchman, almost simultaneously, and quite independently of one another, have made the discovery that the Dead Sea and the entire lower valley of the Jordan, are situated considerably below the level of the Mediterranean Sea."

† *Bulletin de la Soc. de Geogr.* T. x. (1838) p. 84; and Nouv. Ann. de Voy. T. i. 1839, p. 8.

‡ *Bull. de la Soc. de Geogr.* T. xii. 1839, p. 166.

§ M. Russegger has published a detailed account of his labours, in Poggendorff's Annals, 1841, Numb. 5, p. 186.

If the depression of the Caspian Sea and of the surrounding country is inconsiderable, in comparison of the barometrical and trigonometrical measurements made in Palestine, and which, numerically, seem to be very near the truth, it, on the contrary, gains in importance when compared with the differences presented by the different parts of the ocean which are in free communication with each other. In the sequel, in the way of comparison, I shall cite those only which are based on geodœsical levelling, which are altogether worthy of confidence.

The Gulf of Mexico (Mer des Antilles) *and the Pacific Ocean.*

General Bolivar, at my request, in the years 1828 and 1829, engaged the services of an officer of his staff, Mr Lloyd, an American, and of M. Falmarc, a Swede, carefully to survey the isthmus of Panama. In this operation they employed a levelling telescope of Carey's. At the mouth of the Rio Chagres, in the Gulf of Mexico, the difference of the elevation at the full and ebb tide was only 0.16 of a toise; at Panama, on the shores of the Pacific Ocean, it was 3.3 toises. From the survey of Messrs Lloyd and Falmarc, it results that the *mean level* of the Pacific Ocean* is at the most but 0.54 of a toise more elevated than the water of the Gulf of Mexico, but at the time of the neap tide upon the two coasts, the Pacific Ocean is lower than the Gulf of Mexico, to the extent of 1.01 toises. At different hours of the day, then, it is now the one sea, and now the other that is highest. M. Arago has accurately observed, that, in an uncultivated country, beset with difficulties, in going over a line of 33 leagues, and taking 935 observations, the small error of three feet may readily occur; and, consequently, that every proof exists that the difference of level between the two great seas, which communicate by the Straits of Magellan round Cape Horn, is so small as to be inappreciable.† I myself believe that I established, by means of barometrical observations taken from 1799 to 1804, and corrected from the effects of hourly variations, that if any sensible difference existed between the waters of the Mexican Gulf and the Pacific Ocean, this difference was probably not more than three yards (metres). My barometrical observations,‡ as well as the comparison of those of M. Boussingault in 1822 at La Guayra, and those of M. Pentland at Callao of Lima, in 1826, seem to assign even a lower level to the waters of the Mexican Gulf; but the variable influence of capillarity throws a doubt upon the obtained results where they refer to the decimals of millimetres in the column of mercury.

The Mediterranean and the Red Sea.—The problem of the relative height of the Red Sea and the Mediterranean, after having occupied the attention of classical antiquity, has been examined in a very general point of view in Varenius' Treatise on Physical Geography,* for which Newton exhibited so decided a predilection. Anything, however, like accurate measurement, was not made previous to the time of the French Expedition to Egypt. The observations conducted by M. Le Père, have established that the level of the Mediterranean, at the embouchure of the Delta, is inferior by 4.1 toises to the neap tides of the Red Sea, near Suez, and 5.1 toises to the level of full tide. It is probable that the cause of this remarkable difference of level is the *elevation* of the water in the Arabian Gulf to the north of the Strait of Bab-el-Mandeb, and not, as M. de Corancez † has attempted to prove by the hydraulic hypothesis of the reciprocal attraction of the molecules of water, to the *depression* of the eastern portion of the Mediterranean, in which a copious evaporation is not compensated by the volume of water which is poured in by the rivers.

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* Philos. Transact., 1830, p. 84.

† *Notices Scientifiques* of M. Arago, in the *Annuaire* for 1831, p. 319.

‡ Humboldt, *Relation Hist.* T. iii., p. 355–7. Arago, in les Ann. de Chimie, T. i. p. 55 und 64.

The Mediterranean and Atlantic.—This comparison of levels, based upon trigonometrical operations, whose precision surpasses all that has hitherto been published in this kind of labour, is two-fold. The former exhibits the almost insensible difference of the Mediterranean and the Atlantic by the Pyrenees; the latter connects the Zuider-Zee, near Amsterdam, with Marseilles. "Delambre had previously endeavoured to deduce from the great chain of triangles which extends from Dunkirk to Barcelona, the levels of the two seas. The triangles included between Rhodez and the Mediterranean gave, for the vertical height of that town, a result which agrees to a fraction of a metre with the height calculated from the ocean, which was deduced from that portion of the chain interposed between Rhodez and Dunkirk. A trigonometrical survey, executed by MM. Coraboeuf, Peytier, Hossard, and Testu, during the years 1825-1827, and running along the southern frontier of France, supplied all that remained doubtful for the solution of the problem. The station of Crabère occupies nearly the middle of the interval which separates the Atlantic from the Mediterranean. Its height has been calculated by three distinct combinations. One has been conducted by proceeding from the Atlantic and from the Mediterranean to Crabère, passing by the single series of the summits of the triangles which bound the chain on the *south*; a second by selecting exclusively the *northern* summits; and the third and last, by taking the *diagonal* directions, in other words, by taking alternately a northern and a southern summit."‡ I supply below the results of that combination along the chain of the Pyrenees, and of that triangulation which traverses Holland, Germany, and

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* Chap. xiii. prop. 5., chap. xv. prop. 8. I will not attempt to justify the exaggerated ideas of the author upon the volume of water, which the rivers, and especially the Volga, pour into the Caspian, xvi. prop. 5.

† *Itinéraire d'une partie peu connue de l'Asie mineure*, prop. 27.

‡ Arago in the *Annuaire* for 1831, p. 325. Also Mémoires présentés à l'Acad. des Sciences, Tom. iii. p. 81.

France, from Amsterdam to Marseilles.* Geographers and natural philosophers who do not rest satisfied with vague approximative methods, know not how much the admirable labours of M. Delcros have advanced at once hypsometry, geodæsy, and barometrical mensuration.

"*Comparison by the Pyrenees.*—Colonel Coraboeuf, starting from Fort St Ange, upon the shores of the Mediterranean near Perpignan, and from the Fort Socoa, near Bayonne, on the Atlantic, reached the summit of Crabère by four series of points, and found that this point is elevated—

		Metres	
From the Mediterranean by	the Southern Series,	2633.37	Mean = 2633.50 metres.
	the Northern .	2633.99	
	the Diagonals .	2633.87	
	the Diagonals .	2632.79	
From the Atlantic by	the Southern Series,	2632.95	Mean = 2632.77 metres.
	the Northern .	2632.02	
	the Diagonals .	2633.61	
	the Diagonals .	2632.49	

Difference of mean results = to the different levels of the two seas = 0.73 metres.

"This mean discordance in the level of the two seas, being within the limits of probable errors, it may be concluded that the mean levels are nearly identical.

(b) *Comparison through Germany and Holland.*—M. Delcros having measured a geodæsical chain from the Mediterranean to Darmstadt, by Strasburg and Geneva, found—

The height of the gallery of the tower of Darmstadt above the Mediterranean, ...	metres 187.39
Same height by German mensuration, above the Northern Ocean (Zuider-Zee), near Amsterdam, . . .	187.30
Whence it follows that the difference of the mean levels of the two seas, ...	+ 0.09
By the parallel of Bourges from the ocean to Noirmoutiers, measured by Colonel Coraboeuf, the difference is, . .	+ 0.30
Colonel Broussaud, by the mean parallel proceeding from the ocean to Cordouan, near Saintes, is	= + 0.52

"These discordances being all comprised within the limits of the probable errors of geodæsical levellings, prove that no difference exists in the sensible level between the surfaces of equilibrium of these two seas."

We have still to add, and also from a note supplied by M. Delcros, some considerations concerning the rise which recently is alleged to have taken place in the northern part of the Adriatic Gulf. "The Commandant Delcros and Captain Choppin having connected the geodæsical operations executed in France and Switzerland with those effected in Bavaria by General Bonne, have determined the altitudes of Hornliberg, Rigiberg, &c., above the level of the Mediterranean. The Swiss engineers have thus connected these points with those determined in the Tyrol by the Austrian engineers, so furnishing the following results, starting from the Mediterranean:—

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* Taken from a Note which the Commandant M. Delcros has been so kind as give me.

	Metres.		Metres.
Altitude of Kumenberg above the Mediterranean at Marseilles, . . .	= 670.00	Difference	= 7.63
The Austrians give it, above the Adriatic,	= 662.00		
Altitude of Frastenzersand above the Mediterranean at Marseilles, . . .	= 1636.33	Difference	= 8.75
The Austrians make it, above the Adriatic,	= 1627.58		
Altitude of the Fundelkopf above the Mediterranean at Marseilles, . . .	= 2403.80	Difference	= 9.12
The Austrians give it, above the Adriatic,	= 2394.68		
Hence the mean height of the Adriatic Gulf above the Mediterranean at Marseilles.			8.50
The Swiss engineers make it in reference to the ocean, . .			= 7.63

"M. Delcros is persuaded that this difference between the levels cannot exist. Admitting a filling of the Adriatic, from the feeble tides which occur, and from the counter-current that is well known, these effects together cannot amount to 7 or 8 metres. He has been assured that the Swiss engineers, in discussing the partial results inserted in the hypsometry of Austria by Colonel Fallon, have been led to reduce this pretended difference of level from 8 metres to 2. He is not yet informed upon what grounds this reduction is made; but he takes the liberty of expressing a hope that the Austrian Government will direct that the most southern points of Switzerland and Bavaria shall be linked in a manner more satisfactory and accurate with the Adriatic."*

It has appeared to me important to collect in this work all that we know at the present time, concerning the *comparative hydraulic hypsometry* of the waters which belong, and those which do not belong, to the same surface of level. Physical geography can make progress only by grasping the results already obtained, by discussing the bases of numerical elements, and mounting up to those general views which alone give dignity and life to the sciences.†

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* M. Delcros supplies the following results of the correction of the triangles of Bavaria:—

		Metres.
1.	Summit of the tower of Notre-Dame of Munich, . .	= 615.67
	Pavement of the same church, ...	= 518.67
2.	Peissenberg, at the surface, pavement of the church, . .	= 985,14
3.	Benedictenwand, summit of the mountain, . . .	= 1798.96
4.	Wendelstein, summit of the mountain, ...	1843.60

(*Note, September* 1841.)

† From Humboldt's *Asie Centrale*, 1843, vol. ii., p. 301.

The genus Cetochilus belonging to the order Copepoda and the family Pontia of M. Edwards. By Henry D. S. Goodsir, Esq., Conservator of the Museum of the Royal College of Surgeons, Edinburgh. Communicated by the Author, with Plate.

(Continued from p. 104.)

Cetochilus septentrionalis, the species now to be described, is about a line and a half, or two lines long, of a bright red or scarlet colour, and slightly translucent. The body is divided into two great parts, the capito-thoracic, and the abdominal. The superior antennæ are almost obsolete, and are composed of two articulations, the last of which is about four times as long as the first, and is lanceolate. The inferior, or external antennæ, are very long and slender, being longer considerably than the body of the animal. Each of them is composed of twenty-four articulations; the first of these is the largest; the twenty-second and twenty-third are each armed with a very long setum, which is pointed upwards and inwards.

The eyes are two in number, but very minute. The foot-jaws are ten in number. The first (Plate VI. Fig. 12.) consists of two branches arising from a common peduncle; one of those consists of a single articulation only, which is considerably bent, flattened, and which tapers to a point; it is armed with long simple setæ on its superior edge; the other part of this foot-jaw is biarticulate, the first joint being the longest; it is armed with a long single spine at its extremity; the second articulation is about half as long as the first, and its extremity is armed with a number of very long setæ.

The second pair of foot-jaws are connected with the mandibles, and are also composed of two parts arising from one common peduncle, and composed of the same number of articulations as the last, but smaller.

The third pair of foot-jaws are very curious, and if they were free of the setæ would present very much the appearance of a human hand with the index and little fingers concealed.

The fourth pair of foot-jaws are composed of one flattened scale-like joint, with a great number of long setæ arising from its inner edge.

The fifth pair of foot-jaws are composed of four joints, the last of which is very long, and armed on its inner edge with long spines.

The ambulatory legs are ten in number, and are bipartite. They arise from the last five thoracic segments of the body. The peduncular portion of the leg consists of two segments. The external portion of the leg is composed of four articulations, the last of which is long, claw-like, and serrated on its inner edge. The internal edge of the third articulation is armed with a great number of very long spines; the internal portion of the leg is composed of four very short segments; it is as long as the two first segments of the external portion.

The abdominal portion of the body is much smaller than the thoracic, not being nearly so thick, and is generally in an erect position; it consists of six segments, and the last has the extremity armed with two styles, which are each composed of a single plate, the extremity being armed with five very long spines.

The organs of the mouth in this animal, from its small size, I have been unable to make out.

The alimentary canal consists of a simple straight tube. The breathing is carried on doubtless by means of the spines and setæ, which are so numerous on the foot-jaws.

This species is distinguished from C. australis by the two long spines which arise from the twenty-second and twenty-third segments of the external antennæ; the extremities of these antennæ in M. Vauzeme's species having only one spine, or, according to that author, bifid. The foot-jaws also differ in the two species, but especially so in the third pair; these organs in C. septentrionalis presenting very much the appearance of a human hand with the index and little finger flexed, as it were, on the palm, whereas those of C. australis consist of three branches arising from each side of a stem.

SECT. IV.—ON A NEW GENUS AND SPECIES OF CRUSTACEAN.

The animal which is now to be described, is met with occasionally among the *Maidre*. It is solitary in its habits, a single specimen being only seen now and then, owing, however, to its brilliant metallic colours, is easily observed as it shoots along the surface of the water.

The structure of this animal is such as to require the formation of a new generic situation in the family Pontia of M. Edwards. The decision of this question was difficult, owing to the apparently contradictory characters in the structure of the animal. I at one time placed it in M. Edward's restricted genus Monoculus, from the structure of the antennæ and eyes. After a minute examination, however, of several specimens, I was enabled to analyze the structure of the animal more correctly. It forms the, or one of the, connecting links between the two families, namely, Pontia and Monoculus. It is connected more nearly to Pontia by means of the structure of the body, the foot-jaws, the ambulatory legs, the abdomen, and the eyes; to Monoculus by means of the antennæ and the eye.

Upon looking cursorily at the organs of vision, the observer would suppose that the animal was really Monoculus. This arises from the circumstance of the eyes being situated at the superior extremity of a tubular organ, which is apparently for the purpose of assisting the sight. The eyes are situated on the dorsal aspect of the body, near to the anterior extremity of the body, and almost between the origin of the antennæ.

GENUS IRENÆUS.[*] (MIHI.)

A large tubular organ arises from the lower or abdominal surface of the body, in the superior extremity of which the organs of vision are situated; the right antennæ very much swollen a little behind the middle. Foot-jaws ten in number.

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[*] St Irenæus to whom the church of Kilrenny or Irenie in Fife was dedicated.

IRENÆUS SPLENDIDUS MIHI.

Description. The whole animal gives forth a kind of luminous appearance, which is apparently caused by the splendid metallic colours with which it is adorned. The prevailing colours are sapphariue and emerald. The whole length is from about three to four lines; and the body, like those of other animals of the same family, is divided into two great parts, the cephalo-thoracic, and the caudal or abdominal. The head is as large as all the thoracic segments conjoined; the thoracic segments are six in number, and the abdominal are five. The head of this animal is large and rounded superiorly and anteriorly; it projects downwards and forwards in the form of a rostrum. One pair of antennæ arise from the anterior part of the head; they are hardly so long as the thorax, and are very curious.

The right differs from the left in being very much swollen about the middle; it may be divided into three parts; the first or proximal is considerably dilated, and is of an oval shape as far as the sixth articulation, where it becomes much smaller, the four following being only about one-third of the size; the middle division of this antennæ is also very much swollen, but is pyriform, the largest part being proximal, and the contracted distal; this division is six-jointed, and the last joint, which is largest, is deeply serrated on its inner edge. A large muscular or glandular body runs from the base of this division to the proximal extremity of the first articulation of the last division of the antennæ. This last division is composed of three articulations; the first of these is the thickest, and has a deep notch on its inner edge, near to the basal extremity; this articulation is also armed on the same edge, but anterior to the notch, with a number of spines. This curious mechanism seems to be adapted to prehension, but whether it is used for this purpose or not is uncertain. The notch appears to be a kind of hinge or joint, and the spines or serrations on each side of it meet one another, so as apparently to make the hold more firm.

The left antennæ is very different in its appearance from the right; it is composed of twenty-one articulations, is slightly swollen at its base, but gradually tapers to its extremity.

The tubular portion of the eyes is situated immediately behind the rostrum; it is very large, rounded, and bulges at its extremity; it is apparently sessile, is always directed downwards, and the colour is a beautiful dark brown, with a shade of purple. A circular space at the extremity is colourless, and sparkles brilliantly.

The foot-jaws, ten in number, are very similar to those of the Cetochilus. The first is double both branches, arising from one common pedicle, composed of two joints; the external division is largest, and the extremity of the second is armed with two rounded scales, the edges of which are armed with a row of long fringed spines; the internal division is composed of two joints, which are much more slender than those of the external, the extremity of the last being also armed with a series of long and fringed spines or setæ. The second pair of foot-jaws are composed of a peduncle, consisting of two joints, two quadrate scales arising from the extremity of the distal; long spines which are fringed on either side arise from the extremities of each of these. The third pair of foot-jaws are very similar in their appearance to those of Cetochilus. Each of them consists of a large flattened scale-looking body, having a number of projections from its extremity and from either side; each of these projections are armed with long and thickly fringed setæ. The fourth foot-jaw consists also of one piece only which is convex on its outer edge, and concave on its inner, which is armed with very long fringed setæ.

The fifth is small and composed of two divisions, the internal of which consists of one joint only; the external is six-jointed; each of these are armed with spines on the inner edge.

The true ambulatory legs are ten in number, there being five pairs. These legs are all similar in their structure to one another. They consist of two divisions, arising from a common pedicle, which consists of two segments. The external division is composed of four segments, the last of which is ovoid, and is serrated on each side, from each of which serrations there arises a strong spine of moderate length, those from the external edge being shortest; the internal division consists of two segments which are also spined; all of these have more of the characters of spines than setæ, and are totally free of any fringe.

The abdomen is five-jointed, the last segment bearing two oblong quadrate scales from the extremities of each of which arise five long and very thickly fringed setæ.

PLATE IV.*

Fig. 1. One of the third pair of foot-jaws of Irenæus splendidus.
... 2. Tabular portion of eye of same.
... 3. Left antennæ.
... 4. Right antennæ.
... 5. Fourth pair of foot-jaws.
... 6. Fifth pair of foot-jaws.
... 7. Second pair of foot-jaws.
... 8. Caudal segment.
... 9. External organs of generation of male.

PLATE VI.

Fig. 1. Side view of Cetochilus septentrionalis.
... 2. Dorsal view of same.
... 3. First or anterior pair of antennæ.
... 4. Second do.
... 5. Second pair of foot-jaws.
... 6. First do.
... 7. Third do.
... 8. Tail or caudal segment.
... 9. Fourth pair of foot-jaws.
... 10. Fifth pair of do.
... 11. Second pair of ambulatory legs.
... 12. One of the first pair of foot-jaws of Irenæus splendidus.
... 13. Irenæus splendidus.
... 14. Natural size do.
... 15. Dorsal view do.
... 16. One of the second pair of foot-jaws.

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* Description of Plate IV. continued from p. 104.

On the Temperature limiting the Distribution of Corals. By JAMES D. DANA, Geologist of the United States Exploring Expedition. Read before the Association of American Geologists and Naturalists, at Albany, April 29, 1843.

I have before stated to the Association, that the temperature limiting the distribution of corals in the ocean is not far from 66° F. On ascertaining the influence of temperature on the growth of corals, I was at once enabled to explain the singular fact, that no coral occurs at the Gallapagos, although under the Equator, while growing reefs have formed the Bermudas in latitude 33°, four or five degrees beyond the usual coral limits. In justice to myself, I may state here, that this explanation, which was published some two years since by another, was originally derived from my manuscripts, which were laid open most confidingly for his perusal, while at the Sandwich Islands in 1840.* The anomalies which the Gallapagos and Bermudas seemed to present, were dwelt upon at some length in the manuscript, and attributed in the *latter* case to the influence of the warm waters of the Gulf Stream; in the *former* to the southern current up the South American coast, whose cold waters reduce the ocean temperature about the Gallapagos to 60° F. during some seasons, although twenty degrees to the west, the waters stand at 84° F. *Extratropical* currents, like that which flows by the Gallapagos, are found on the western coasts of both continents, both north and south of the Equator, and *intratropical* currents are as distinctly traceable on the eastern coasts.† In consequence of these currents, the coral zone is contracted on the western coasts and expanded on the eastern; it is reduced to a width of sixteen degrees on the western coast of America, and of but twelve degrees on the east coast of America; while in mid-ocean it is at least fifty-six degrees wide, and about sixty-four degrees on the east coast of Asia and New Holland. The peculiar bend of the east coast of South America carries off to the northward much of the usual south intratropical current, and it is therefore less distinct in its effects than the *northern* intratropical or Gulf Stream.

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* The publication here alluded to, we understand, refers to an Article, by Mr J. P. Couthouy, which appeared last year in the Boston Journal of Natural History.

† The existence of these great oceanic currents was first pointed out to me by our distinguished meteorologist Mr William C. Redfield, who kindly furnished me with charts of the same before the sailing of the Expedition.

We have hence the remarkable fact, that the coral zone is fifty degrees wider on the eastern than on the western coasts of our continents. Such is the effect of the ocean currents in limiting the distribution of marine animals. These facts will be brought out more fully in the reports of the Exploring Expedition. The important bearing of these facts upon the distribution of fossil species is too apparent to require more than a passing remark. The many anomalies which have called out speculations as to our globe's passing through areas in space of unequal temperatures, are explained without such an hypothesis. Instead of looking to space for a cause, we need not extend our vision beyond the coasts of our continents.—*American Journal of Science and Arts*, vol. xlv., No. I. p. 130., July 1843.

On the Areas of Subsidence in the Pacific, as indicated by the Distribution of Coral Islands. By James D. Dana, Geologist of the United States Exploring Expedition. Read before the Association of American Geologists and Naturalists, at Albany, April 29, 1843.

The theory of Mr Darwin, with regard to the formation of atolls, or annular coral islands, has been fully confirmed by the investigations of the Exploring Expedition; but his regions of subsidence and elevation, and the conclusion that these changes are now in progress, appear to have been deduced without sufficient examination. Observations at a single point of time cannot determine whether such changes are in progress; they can only assure us with regard to the past. A series of examinations, for years in succession, is necessary to enable us to arrive at the grand deduction, that the land in any part of our globe is now undergoing a gradual change of elevation.' The views of Mr Darwin, respecting the rise of the South American coast, as well as that of the Pacific and East Indies, may well be received with some hesitation. According to my own observations, regions, in which his theory would require a subsidence, have actually experienced an elevation at some recent period. I might instance several examples of this elevation in various parts of the Pacific. Suffice it to say here, that I found nothing to support the principle laid down by him, that islands with a barrier reef are subsiding, while those with only a fringing reef are rising; indeed, facts most stubbornly deny it. Without entering upon the discussion of those facts, which, as they will appear in the government publications, I am not at liberty to dwell upon here, I propose to point out what are the regions of subsidence which the coral islands in the Pacific indicate as having been in progress during their formation.

Before proceeding, I may be excused for adding here a few words in explanation of Mr Darwin's theory, with regard to the formation of coral islands. He rejects the unfounded hypothesis that coral islands are built upon the craters of extinct volcanoes, and proposes the following theory in its stead, which is supported by a minute as well as general survey of the facts. The coral belt or atoll, he supposes to have been originally a barrier reef around a high island, like the reef round many islands in the Pacific. When the reef commenced, it could not have been extended to a lower depth than 100 or 120 feet, for this is the limit of the reef forming corals. But if the island gradually subsided—so gradually that the corals could by their growth keep themselves at the surface, the reef might finally attain any thickness, according to the extent of the subsidence. In this manner, subsidence might finally submerge the whole island; and leave nothing but the reef at the surface. Mr Darwin points to instances in which only the mountain tops now remain above the ocean. Carry the process a little farther, and we have the coral belt surrounding its little sea—the usual condition of the coral island.

This theory, as is seen, supposes extensive subsidence. And so, we remark, must every theory; for, without it, we could only have reefs 120 feet in depth, instead of the great thickness they are believed to possess. It is my present object to fix the area of this subsidence, and suggest something with regard to the extent of it in different parts of the ocean. On examining a map of the Pacific, between the Sandwich Islands and the Society group, we find a large area just north of the Equator with scarcely an island. To the south, the islands increase in number; and off Tahiti, to the northward and eastward, they become so numerous, and are so crowded together, as to form a true archipelago. They are all, too, coral islands throughout this interval. This, then, is a rather remarkable fact in the distribution of these islands. But let us look farther.

If we draw a line running nearly E.S.E., from New Ireland, near New Guinea, just by Rotumah, Wallis's Island, Samoa or the Navigators, the Society Islands, and thence bending southward a little, to the Gambier group, we shall have all the islands to the north of it, with two or three exceptions, purely coral, while those to the south are very generally high basaltic islands. These basaltic islands are bordered by reefs, and these reefs are most extensive about the islands nearest this line. In the Feejees, the north-eastern part of the group contains some coral rings, while the north-western consists of large basaltic islands with barrier reefs.

Again, to the north of this boundary line, the islands *farthest* from it are usually small, in many instances mere points of reef, a fraction of a mile in diameter, while some of the coral islands *near* the same line are thirty or forty miles in length.

Now, a growing coral island or atoll will gradually become smaller in diameter as subsidence goes on, and by the same process must finally be reduced to a mere spot of reef, or, if the subsidence is too rapid, that is, more rapid than the growth of the coral, the island will become wholly submerged and leave nothing at the surface.

On these principles I base my conclusions. Along the equator, as explained, there is a large area containing few islands, and these small, while farther north, the coral islands are numerous and large: Is this not evidence, that the subsidence was either more rapid or carried on for a longer period in the former region than in the latter, where they are numerous and large?

Near the boundary line pointed out, stand some of these coral rings, enclosing mountain tops as islets,—as at the Gambier group. Does not this indicate, that the subsidence was less here than among the islands *purely coral* to the north? And greater, than south of the line, where the reefs are more contracted, and the high islands larger and more elevated? Washington Island (coral), in lat. 5° N., is the last spot of land as we recede from our boundary line to the north-east. Beyond is a bare sea, to the Sandwich Islands. Is not this an area where the subsidence was too rapid for the corals to keep the islands at the surface?

It appears that, during this era, the Pacific from 30° N. to 30° S., and perhaps beyond, was one vast region of subsidence: that subsidence took place most rapidly over the bare area between the Sandwich Islands and the Equator, and less and less so, as we go from this to the south-south-west. At the boundary line pointed out, it was not sufficient to submerge many of the mountain summits; and south of this, the effect was still less.

This area covers at least 5000 miles in longitude, and 3000 in latitude. The seas about the north-west coast of New Holland, shew by their reefs a contemporaneous subsidence, and they should probably be included, as well as some parts of the East Indies. Fifteen millions of square miles is not, then, an over-estimate of the extent of the region that participated in this subsidence.

The region of greatest subsidence lies nearly in a west-north-west line, for we may trace it along by Washington Island, far towards the Arctic Coast. The whole broad area of subsidence has nearly the same direction; for this is the course of the boundary line we have laid down as separating the high basaltic and the low coral islands. It is highly interesting to observe, that the trend of the principal groups of islands in the Pacific corresponds nearly with this course. The low or coral Archipelago, the Society Islands, the Navigators, and the Sandwich Islands, lie in the same general direction, nearly west-north-west, and east-south-east. It should be remarked, that the Sandwich group does not contain merely the seven or eight islands usually so-called; eight or ten others stretch off the line to the north; some, small rocky islets, and others, coral, and the whole belong evidently to one series. I will not say that there is a connection between the trend of these groups and the area of subsidence, yet it looks much like it.

A further point may be worthy of consideration. The Sandwich group consists of basaltic islands of various ages. The island at the north-west extremity, Tauai, is evidently more ancient than the others, as its rocks, its gorges, and broken mountains, indicate. By the same kind of evidence, it is placed beyond doubt, that igneous eruptions on these islands continued to be more and more recent, as we go from the north-west to the south-east: at the present time the great active volcano is at the south-east extremity of Hawaii, the south-east island. The fires have gradually become extinct from the north-westward, and now only burn on the south-west point of the group. At the Navigators, and I believe also at the Society group, the reverse was true; the north-west island was last extinct. Is there any connection between this and the fact, that low islands are numerous north-north-west of the Sandwich Islands, and south-south-east of the Society? Does it indicate any thing with regard to the character or the subsidence in these regions?

The time of these changes we cannot definitely ascertain; neither when the subsidence ceased, for it appears to be no longer in progress. The latter part of the tertiary and the succeeding ages may have witnessed it. Although I am by no means confident of any connection, yet for those who would find a balance-motion in the changes, I would suggest that the tertiary rocks of the Andes and North America indicate great elevation since their deposition; and possibly during this great Pacific subsidence, America, the other scale of the balance, was in part undergoing as great or greater elevation.

But why if the Western American coast was rising, do we find no corals on its tropical shores to indicate it? The cold extratropical currents of the ocean furnish us with a satisfactory reply.—*American Journal of Science and Arts,* vol. xlv. No. I. p. 131, July 1843.

An Account of Experiments with Thermo- and Hydro-Electrical Currents, with an examination of the Metals exposed to Thermo-Electric action. By Mr R. ADIE of Liverpool. Communicated by the Author.

The experiments detailed in the following communication, had their origin in a desire I felt to test a hypothesis, which regarded *heat* as a binary compound, composed of latent heat and electricity. The study of thermo-electricity had already induced M. Becquerel to hold a view not very dissimilar, viz. that under some circumstances heat was resolved into the two electricities; and again, that the two electricities could combine, and produce heat. From the same source I hoped to derive proof or disproof of the theory that latent heat and electricity could unite, but the experiments had not proceeded far, before I began to despair of obtaining evidence on this head; nevertheless, the results given by the first attempts appeared to me sufficient to encourage further examinations, when they were conducted without reference to the hypothesis in question. I then resolved to follow only the experiments, and it is with the details of that part of them which appears to me to be worthy of the attention of your readers, that this paper is now submitted.

I was desirous to obtain a thermo-electric battery, which would produce as powerful a current as possible, by the action of sun and sky, without the aid of differences of temperature sustained by artificial means. M. Melloni's small needles of antimony and bismuth are known to form a thermo-electrical battery of great sensibility, but for a permanent current sustained solely by the temperature due to different radiations through the atmosphere of the two extremities of the bars, I found longer pieces of antimony and bismuth far more effective. On varying the length of these pieces of metal from 1 to 16 inches, I found 8 inches to develope the full power of a single pair.

When the number of bars of antimony and bismuth are gradually increased, the quantity of electricity indicated by the galvanometer follows in the same direction, but from each successive addition of a pair of bars, the quantity of electricity derived from that addition decreases, as the number of bars, or length of the electrical current, increases; so that, when 2 sets of 30 pairs of M. Melloni's bars are combined together, to form one battery of 60 pairs of antimony and bismuth needles, there is a far greater gain in quantity of electricity by this addition of 30 to 30, than we gain from the next similar addition of 30 to the circuit of 60 pairs.

Beyond 90 pairs of antimony-bismuth bars of the size used by M. Melloni, combined in a single battery, I have not been able to gain any augmentation to the electrical current. If 8-inch bars be employed, and it is sought to increase the quantity of electricity from a single battery to its highest point, this will be found to be attained by 11 pairs of bars, where the electricity circulates through 176 inches, or nearly the same length of the combined metals as in a battery of 90 pairs of 1-inch bars; hence, in constructing thermo-electrical batteries, the number of bars capable of producing the greatest effect, is dependent on the length of the bars chosen. Again, when we desire to produce the greatest effect from a number of bars, whether in a single battery, or arranged in several batteries acting together, the principle given by Professor Daniell for arranging hydro-electrical cells, will hold good for thermo-electricity.* "From these experiments it appears, that the most advantageous adjustment of active force and resistance is in the series of 10 single cells, and that the largest amount of work which can be derived from 20 cells, is when they are arranged in two series of 10." 20 pairs of antimony and bismuth bars, 8 inches long, to give the largest amount of work, should be disposed in two series of 10 each.

On combining a number of series of thermo-electrical batteries together, the quantity of electricity supplied by the whole is a simple multiple of the number; I have found in 36 series with 80 inches circuit for the electricity, the quantity to increase regularly with each successive addition of a series to those already in action. The case here is different from bars combined to form a single battery; in the latter the whole electricity generated has to circulate through a greater length of metal on each addition of a pair of bars, while the resistance to the conduction of the fluid progresses by this change in a greater ratio than the quantity gained by each successive addition to the number of elements. In the former, where different series of thermo-electric batteries are placed so as to act alongside one another, the length of the bars of metal, through which the electricity passes, remains constant; hence 2 series supply twice the quantity of one, 3 series three times the quantity of electricity, and so on.

When a pair of antimony and bismuth bars soldered together had their joint, and a portion of the bars for about 2 inches within it heated by the sun, the electrical current generated was found to be more powerful than when the joint only was exposed to the solar rays; this led me to try arrangements of bars with pieces projecting beyond their joints, antimony being a better conductor of heat than bismuth, was selected, and arranged as the projecting metal.

I have found this kind of battery superior to those where all the metal surface engaged radiating forms part of the electrical circuit, and I believe it to be solely due to the large radiating surface placed beyond the electrical circuit, where it effects the required difference of temperature betwixt the joints, with a short path for the thermo-electricity developed.

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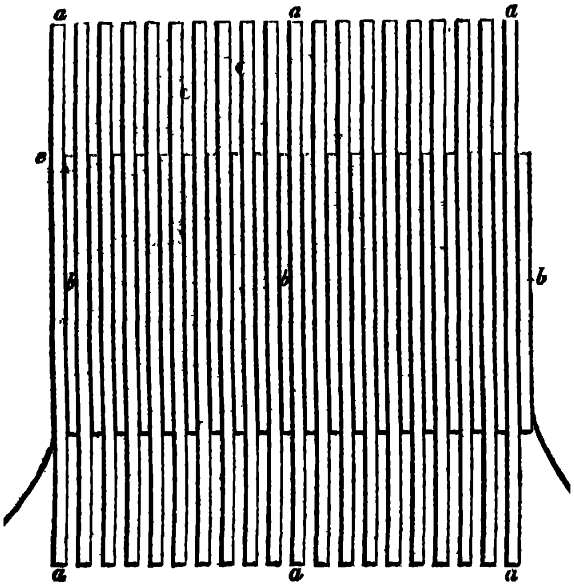
* See Introduction to Chemical Philosophy, p. 466.

The annexed figure represents a thermo-electrical series of antimony-bismuth bars, combined to be used for developing thermo-electricity by solar or astral radiation.

Fig. 1.



a, a, a, a, represent 20 bars of antimony, 8½ inches long, by three sixteenths of an inch square. *b, b, b,* 20 bars of bismuth, 4½ inches long, three-sixteenths of an inch square.

Slips of brown paper coated with shell-lac, intervene betwixt each bar, so as to insulate the metals in every part, excepting their soldered joints.

The shell-lac also serves as a cement to bind the bars together.

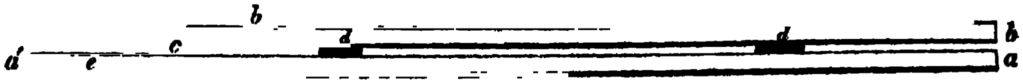
c, the pole attached to the antimony, *e,* the first joint, *d,* the pole attached to the bismuth.

In this arrangement, the electricity circulates through 40 bars, 4½ inches long each, or 180 inches is the length of the combined metals.

The thermo-electric battery (fig. 1,) can be employed as a meteorological instrument to shew the amount of radiation to or from the earth; for this purpose it has to be fixed in an exposed situation, with the lower half screened from the sun and sky; wires soldered to the poles are then led to a galvanometer fixed inside of a house, when the wires are 15 feet long; the clear summer's sun turns the magnets of the galvanometer nearly to a right angle; while astral radiation, under favourable circumstances, deflects them in an opposite direction 25 or 30 degrees. The arrangement described is remarkably sensitive to thin vapours passing over the sky; and even with the galvanometer, 40 feet distant from the battery, the indications are sufficient to furnish a scale which will enable an observer on inspection to judge of the rate of radiation to or from the earth.

The battery with projecting radiating ends appeared to me to admit of application to generate currents of the lightest thermo-electrical intensity; the annexed figure shews the side view of a 4-pair battery arranged to be excited by flame.

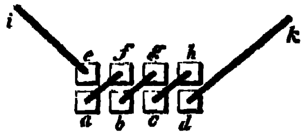
Fig. 2.



a, a, Antimony bar 8½ inches long; *b, b,* bismuth bar 6 inches long; *c,* the soldering of pure bismuth exposed to a heat a little lower than the melting temperature of this metal.

d, d, Slips of wood one-twentieth of an inch thick, coated with shell lac, and inserted betwixt the bars to keep them from touching. Four pairs of these bars are placed side by side, and kept apart by slips of wood like *d, d.* Figure 3 represents the end view of the 4 combined pairs, with the solderings necessary to complete the electrical circuit; *a, b, c, d,* the extremities of the antimony bars farthest from the exciting flame; *e, f, g, h,* the corresponding extremities of the bismuth bars; *af, bg, ch,* solderings to complete the electrical circuit; *e i,* the pole attached to bismuth bar; *d k,* the pole attached to antimony bar. Weight of the whole, 8 ounces.

Fig. 3.



The advantage derived from placing the antimony bars side by side is, that a small flame serves to excite the whole. The parts *a, e* (figure 2) of the antimony bars, are enveloped in a blue de-oxydizing flame, where they attain a temperature corresponding to the first degree of redness, barely perceptible in the dark, and maintain at the soldering *c* (figure 2) a heat little lower than the melting temperature of bismuth. Thus, betwixt the two extremities of the pile, a difference of temperature equal to 300° of Fahrenheit, may be sustained by means of a small gas flame to supply a powerful and equal current of thermo-electricity for months.

9. In performing a number of chemical decompositions by the thermo-electricity derived from the battery, figures 2, 3, they were found to be subject to irregularities, which at first proved very perplexing; during 4 months that the battery was in constant action, there appeared to be a gradual loss of decomposing power. The action was commenced at the end of the hottest part of the summer, and as the weather approached its winter temperature, the difference betwixt the hot and cold ends of the bars had augmented about 25 degrees, consequently the battery was generating a current of higher intensity (for the electro-motive force has been proved to vary with the temperature). Any gain, however, from such a source, is entirely annihilated, if the temperature of the decomposing cell be allowed to follow the weather. Electrolysis by thermo-currents, from the low intensity of the electricity, are keenly sensitive to any

change in the affinities of the substances electrolized; consequently, they become a delicate measure of variations in affinities.

10. Where copper poles immersed in a sulphate of copper solution are used, for an electrical current to oxydize and deposit the metal, the affinity of the copper for oxygen rises with the temperature, while, at the opposite pole, it appears not to retain it more firmly; but in working with my feeble currents, I have found that a lower intensity will suffice to oxydize copper at the positive pole, than is necessary to precipitate it as a metal on the negative pole.

11. As the thermo-electrical current from the battery, figures 2, 3, could be relied on for regular action during a period of steady weather, I made experiments of two days' duration each, to ascertain the changes in the quantity of copper oxydized at the positive, and precipitated at the negative pole, at different temperatures, by a constant electro-motive force.

The mean of two experiments each for 55, 65, 130, and 200 degrees of Fahrenheit, gave quantities corresponding to the numbers 9, 16, 110, 320, the effect increasing in an accelerated ratio. In the experiments conducted in a voltameter at 55°, there is no copper precipitated on the negative pole, but the quantity oxydized at the positive pole measures the extent to which water has been electrolized.

Hence, in manipulating with electrical currents of low intensity, it is essential to attend to the temperature of the decomposing cell, for the force which separates the particles of oxygen from hydrogen is made up of the electrical power and the affinity of the metal of the pole (in the above cases copper), for oxygen in its nascent state, platinum poles, used with the same electro-motive force, are unable to effect any decomposition in acidulated water at temperatures within our command.

Where the temperature of the voltameter was allowed to change with the season, the same cause which exalted the intensity of the thermo-current removed a force acting in conjunction with it, producing a rapid diminution in the extent of the decomposition; therefore, the loss of affinity of copper for oxygen proceeds in a more rapid ratio than the gain to the intensity of the current, from a greater difference of temperature betwixt the two extremities of the pole.

Hence the apparent loss of power in the thermo-battery, figures 2, 3, during the 4 months' action, and on restoring the decomposing cell, to its former temperature, electrolysis assumed the activity given at the commencement.

12. It must be evident that, in pushing decompositions by thermo-currents to their highest point, assistance may be derived from a high temperature in the voltametre. Iron, for example, decomposes water without any aid from electricity at a red heat; at 60° Fahrenheit it requires the assistance of a powerful thermo-electric current to disunite the elements of water; betwixt these two temperatures there is an extensive range of affinity, requiring, at the different points of the scale,

an electro-motive force, which, added to the affinity of the iron for oxygen at these points, is sufficient to effect the decomposition. Hence it follows, that the feeblest electrical currents with which we are acquainted, may be made to decompose water when assisted by the affinity of the metals forming the poles for the nascent oxygen, eliminated by platinum and a more powerful current.

13. Although I have never succeeded with any decomposition by platina poles, and a thermo-current at the highest temperature, I could command, with oxydizable metals, as performed by Mr Watkins,* the battery, figures 2, 3, oxydizes and deposits metallic copper in a voltameter at 200° Fahrenheit, at the rate of 21 grains per day, the difference in weight betwixt 2 equal copper wires after 24 hours' action being 42 grains; so that, in 164 days, a quantity of copper equal to the whole weight of the battery can be deposited, while, to all external appearance, the bars which developed the force essential for this powerful chemical action have undergone little change.

14. The metallic salts, when placed in a voltameter with poles of the same metal as their base, yield readily to a thermo-current; under such circumstances, there are a great number of metallic compounds decomposable by this force, where the difference of temperature betwixt the two ends of the battery might serve to measure the relative power of the affinities which unite the elements of the electrolice.

14. The cyanuret of silver in solution, now so much used in electroplating, undergoes electrolization with a thermo-current, derived from a single pair of antimony and bismuth bars, excited by radiating to a clear sky. The silver appears in the usual frosted form on the negative pole, and I believe this metallic deposit can be obtained at lower temperatures and with feebler currents than any other metal. Copper poles, and iron poles in a solution of water acidulated with sulphuric acid, give as distinct evidence of decomposition of water by feeble currents, as we obtain with the cyanuret of silver and pure silver poles; with copper poles, the black oxide of copper is obtained, then the fluid is slightly tinged by the formation of the sulphate of copper; with iron poles, the green tinge from the sulphate of iron appears. But for all of them, it is essential thoroughly to exclude the atmosphere, otherwise a more active source of chemical action will be present, which will render it impossible to watch the effect of the thermo-electrical current. For these minute indications, I have found it necessary to keep the bars in action from three to six months before the evidence of the decomposition could be relied on. At their commencement, the galvanometer, when applied to form part of the circuit, shewed that minute quantities of electricity passed; but as Dr Faraday had found that fluids were capable of conducting electricity to a small extent without decomposition, this did not appear

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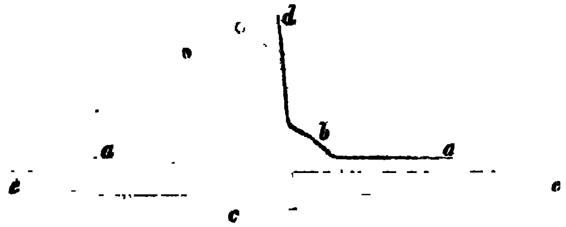
* Philosophical Magazine, vol. xii. p. 341.

to me sufficient to establish the fact of electrolysis being performed by currents of the lowest intensity.

15. The annexed figure 4, shews the form of voltameter suited for excluding the atmosphere, or for working under pressure.

Fig. 4.



a, *a*, A piece of glass thermometer-tube of round bore, blown into a bulb at *b*, *c*. *d*, A small capillary tube drawn out from the bulb, and hermetically sealed, after it has been used to fill the bulb with the fluid under trial. *e*, *c*, Poles cemented or fused into the glass tube at *a*, *a*.

With silver poles, I have found to fuse the glass round the silver the most convenient, but with oxydizable metals a cement must be employed, for the pole would be coated with oxide of the metal, if fusing was attempted.

16. The decompositions performed by a single thermo-couple, acting for a long time, can be effected quickly, comparatively speaking, by the arrangement figure 1, exposed to the sky. At the beginning of May, a single day of clear sun made a distinct deposit of silver on the negative pole, in a voltameter, fig. 4, with silver poles, and filled with cyanuret of silver. In this experiment, the decomposing cell is exposed to the sun's rays to raise its temperature, and thus exalt the affinity which acts in conjunction with the electrical force (10.). Again, with copper poles in acidulated water, arranged similar to the foregoing experiment with silver poles, the astral radiation, for 8 hours of a clear summer night, temperature 60°, developes a thermo-electrical current which coats with a thin film of the black oxide of copper the positive pole, while the negative pole retains its original brightness.

17. The thermo-electric battery, fig. 1, arranged as described (7.), had a voltameter, fig. 4, with a solution of cyanuret of silver, and silver poles, attached by a pair of wires 40 feet long each; these wires allowed the voltameter to be maintained at a nearly uniform temperature of 90° in a sand-bath; the experiment commenced on the 1st of March and continued for two months; the silver poles were adjusted at the beginning to be exactly of the same weight; when weighed at the conclusion, there was a difference of $\frac{1}{10}$th of a grain, with a distinct appearance of silver deposited on the pole attached to the antimony extremity of the thermo-battery, which is the negative pole for the current derived from astral radiation. How the electrical current from this source should exceed the quantity supplied by solar radiation during March and April, has been to me a matter of some surprise, and even for May the nocturnal

current seems to be the most powerful; but this may be occasioned by the predominance of heavy cloudy days during the present month.

As the existence of electrical currents through the metalliferous veins of the earth, has been demonstrated by Mr Fox, the details of these cases of minute decomposition by feeble currents, may be useful to assist us to comprehend the sources of change in the form and deposition of matter in the great laboratory of Nature.

18. Since the experiments given in 14-16 prove the deposition of metallic silver to be far within the range of thermo-electrical currents derived from astral and solar influence, the question naturally arises, whether, in addition to the application of the battery, fig. 1, to meteorological purposes, described (8.), it may be so arranged as to deposit silver, through the calorific power of the summer's sun, and thus supply a scale wherewith to measure or compare the solar heat of one season with that of another.

For our climate, to gain a range sufficiently great, currents derived from several series of batteries must be passed into one voltameter, fig. 4, which, like the upper half of these batteries, must be exposed to sun and sky. The experiment in 14. shews that the current in an opposite direction can remove part of the silver deposited by day; at temperatures below 60°, I have always found the results of electrolysis so minute, for currents derived from astral or solar influence, that this source of error is imperceptible in the result; astral radiation acting on a battery, fig. 1, for the three winter months, did not deposit silver to weigh $\frac{1}{100}$th of a grain, yet with a magnifier the usual frosted appearance of the deposited metal could be distinctly seen. In a warm climate, there can be little doubt that the sun will deposit enough of metal for the required scale. After the experience of 8 months with the action of this instrument in our latitude, I believe that it will prove useful to those engaged with inquiries into the extent of radiation in different climates. For example, a battery, with its attached voltameter, might be placed in a high alpine station, and left untouched for a year.

(*To be continued.*)

Zoological Summary of the Extinct and Living Animals of the Order Edentata. By Professor Owen.

The light which is thrown on the nature of animals belonging to extinct species by the comparison of their fossil remains with recent skeletons, is very often reflected back, so as to elucidate affinities of existing species which were before obscure, and which must otherwise have remained subjects of debate and doubt. Of the happy influence of Palæontology in the resolution of such problems in natural history, the present application of the osteology of the Megatherioids affords a good example.

The genera *Bradypus* and *Cholæpus* have been regarded by all zoologists as forming one of the most anomalous and isolated groups in the mammiferous class, of which no other proof is needed than the fact, that whilst Cuvier, in the "Règne Animal," has placed the Sloths in the lowest order of Unguiculata, his successor* in the celebrated French school of zoology has seen reason for raising to the highest, or quadrumanous order, agreeably with an old opinion of Linnæus.

Our present knowledge of the extinct Megatherian quadrupeds leads us to contemplate the natural affinities of the Sloths from a vantage ground not attained before, yet essential to a correct and comprehensive view of them. The tardigrade and scansorial Edentata appear to the classifier conversant only with existing forms as a very restricted and aberrant group; but they may now be recognisable by the palæontologist as the small remnant of an extensive tribe of leaf-devouring and tree-destroying animals, of which the larger extinct species were rendered equal to the Herculean labours assigned to them in the economy of an ancient world, by a gigantic development of the unguiculate type of structure, combined with such modifications as unequivocally demonstrate that they were at the lowest step of the series of Mammals furnished with claws, and that they completed the transition to the ungulate division of the class.

It harmonizes well with this general view of the affinities of the Megatherioid quadrupeds, that whilst they brought the unguiculate type, both by modifications of structure and predominance of size, most closely to that of the great-hoofed herbivora, they likewise were, of all the quadrupeds provided with formidable claws, the most strictly vegetable feeders.

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* M. de Blainville, *Prodrome d'une Nouvelle Zooclassie*, 1816; quoted by the author in his recent splendid *Ostéographie*, in which he adduces the following osteological characters as common to the Sloths and Quadrumanes:—"Ce sont des Primates:—Par l'état complet de l'avant-bras; la rotondité de la tête du radius; la mobilité du carpe sur l'avant bras.—Par l'état également complet de la jambe dans ses deux os; la grand mobilité du tarse sur les os de la jambe.—Par la forme generale du tronc, presque sans queue, large et déprimé plutôt que comprimé à la poitrine;—par la largeur du bassin."—*Ostéographie de Paresseux*, 4to, 1840, p. 54.

And, if we have reason to view the structural differences or superadditions which the Megatherioids present, compared with the Sloths, as being the necessary consequences of an identity of diet in quadrupeds too bulky to climb, and therefore requiring new powers for the attainment of the foliage, such an interpretation of the peculiarities of their organization, whilst it confirms the close mutual affinities between the great extinct and small existing phyllophagous Unguiculata, at the same time indicates unerringly the true natural affinities of the whole of this great tribe to the other groups of Mammalia.

It would border upon the ridiculous to advocate the claims of the Mylodon to the Quadrumanous order, because its thorax was wide rather than deep, its muzzle broad and truncated, its pelvis expanded, the head of the radius round and apt for rotation, the inflection of the carpus and tarsus free, the long claws prehensile, and the diet exclusively vegetable. Yet the claims of the Megatherians to be associated with the Apes and Lemurs, are, on those grounds, equal with those of the Sloths.

The only modifications in the small tardigrades which might mislead a naturalist into exaggerating the importance of the characters just cited, are due, in fact, to the absence of the characters of the phyllophagous Edentata, by which the Sloths are made inferior to the Megatherioids, without being thereby especially approximated towards the Quadrumana: such, for example, as the removal of the ungulate digits, the loss of the mobility of certain joints in the hands and feet, the diminution of the bulk of the body, and the incomplete clavicles in one of the species.

It is most probable that the Megatherioids, like the Sloths, give birth to a single and unusually large foetus; but in that case, they would coincide in their uniparous generation with the Elephant and Whale, as much as with the Ape. If their uterus was undivided, as in the Sloths, they would agree with the Armadillos, as well as with the Quadrumanes. The pectoral mammæ of the Dugong and Elephant shew the insufficiency of this character in determining the natural affinities of a mammal, if we assume that the Megatherioids, like the Sloth, resembled the Primates in the position of the lactiferous organs.

In the lowest of the Quadrumana, as the Mydas Monkey,* the brain, though smooth and almost as devoid of convolutions as in the bird, is yet characterized by the large proportional size of the cerebral hemispheres, which extend a considerable way over the cerebellum. In the Sloths the cerebellum is almost left exposed, and in the Megatherioids must have been quite uncovered by the cerebrum, which was of as small proportional size as in the Ant-eaters and other Edentata. The forward inclination of the occipital plane which the Sloths and Megatherioids present, in common with most other Edentata, is a character manifested by no true quadrumanous animal.

The dental system has evidently reached its lowest condition, amongst Mammalia, in the order Edentata. As respects the proportion of the order, comprising the true Ant-eaters and Pangolins, to which the term "Edentulata" was originally and restrictively applied by Brisson, that term is quite appropriate, and it would have been well if its signification had not been extended to so many species to which it is inapplicable. The Orycterope, or Cape Ant-eater, for example, has molar teeth; some of the Armadillos possess, in addition to their molars, one or two teeth, which may, from their position, be termed incisors; and the two-toed Sloth has teeth which, by their size and shape, merit the name of canines; but whatever be the position, shape, or use of the teeth, in no Edentata species of the Cuvierian system does the enamel enter into their composition.

The modifications in the intimate structure of the teeth, which are extreme, and peculiar to the quadrupeds of this order, may be regarded as another indication of the low ebb to which the development of the dental character has sunk, now variable, and, as it were, flickering before its final disappearance.

In the Orycterope we find, strangely repeated, a microscopic structure characteristic of the teeth of the Ray and Saw-fish,* very different from any modification in the teeth of other Edentata, or of other Mammalia. The intimate structure of the teeth of the Megatherioids and Sloths, is quite as peculiar to them among mammalia; but this modification has not been observed in any other class of vertebrate animals.

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* See my paper on the Brains of the Marsupial Animals, Phil. Trans., 1837, Pl. V., Fig. 2, p. 93.

This structural peculiarity of the teeth, and their continual growth in the Sloths, are characters which, independently of the total absence of incisors, and diminished number of molars, form an essential objection against their approximation to the quadrumanous order;† and the value of these differential characters is greatly increased by their close repetition in the teeth of all the large extinct Megatherioid animals, which, whilst essentially related to the existing Sloths in other parts of their organization, approximate in those modifications by which they differ from the Sloths, not to the Quadrumana, but to the Ant-eaters, and in a minor degree to the Orycterope and Armadillos; thus demonstrating by their differences, as by their resemblances, the essential relations of the Sloths to the Edentate order of Mammalia.

The degradation of the armature of the jaws in this order produces, especially in the truly edentulous Ant-eaters, a resemblance to the class of birds in one of their best marked characters; and amongst the implacental Edentata, we find the jaws themselves assuming the form of a duck's bill in the Ornithorynchus.

It may be observed of the Sloths, that they illustrate this affinity or tendency to the oviparous type by the supernumerary cervical vertebræ supporting false ribs, and by the convolution of the windpipe in the thorax, in the three-toed species; by the lacertine character of three and twenty pairs of ribs in the Unau; and by the single excrementory or cloacal outlet, by the low cerebral development, by the great tenacity of life and long enduring irritability of the muscular fibre, in both species.‡ Most interesting, therefore, becomes the discovery, that in one of the huge extinct Sloths, another character, heretofore deemed peculiar to the class of birds, should have been repeated, viz. the bony confluence of the last dorsal and the lumbar vertebræ with the sacrum. All these indications of a transition to a lower class harmonize with the Cuvierian view of the zoological position of the Sloths, as members of one of the lowest and most aberrant orders of Mammalia; and all oppose themselves to the promotion of the Sloths to the Primates, and to their separation from the terrestrial Edentata, which afford in the Ant-eaters and Pangolins, the Echidna and Ornithorynchus, so many additional retrograde steps towards the Oviparous classes.

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* Report of the British Association, 1838, p. 145.

† M. de Blainville admits, that the character of the dental system,—"Ce système dentaire plus ou moins incomplet," loc. cit. p. 58,—is an indication of their affinity to the Edentata.

‡ "Cor motum suum validissime retinebat, postquam exemptum erat e corpore, per semihorium;—Exempto corde cæterisque visceribus, multo post se movebat

It would be tedious to reiterate the special and gradational affinities of the Mylodon and its congeners to the different families of the Edentate order, since these have been so fully elucidated in the comparisons of the several parts of their skeletons. They establish the general conclusion, that the existing arboreal and extinct terrestrial Sloths constitute a primary division or tribe of the order BRUTA or EDENTATA, equivalent to the tribe Loricata or Armadillos, and to the true Edentata, or the Ant-eaters and Pangolins.

The teeth and jaws give the essential character, and govern the aliment of the new primary group, of which the name *Phyllophaga*, here proposed, indicates the characteristic and peculiar diet.

The characters of the tribe, of its families and genera, and of the extinct species especially noticed in the present Memoir, are given in the subjoined Synoptical Table.—Owen on the Mylodon robustus, p. 162.

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et pedes lente contrahebat sicut dormituriens solet."—*Pison, Hist. Bras.* p. 322, quoted by Buffon, who well observes, "par ces rapports, ce quadrupède se rapproche non seulement de la tortue, dont il a déjà la lenteur, mais encore des autres reptiles, et de tous ceux qui n'ont pas un centre du sentiment unique et bien distinct."—*Loc. cit.* p. 45. The endowment of a persistent formative organ of the teeth indicates another property in the Megatherioid animals, by which they would resemble the cold-blooded Reptiles, viz. longevity.

Conspectus Familiarum, Generum, et Specierum, Brutorum frondes carpentium.

ORDO—BRUTA, Linnæus, Fischer (*Edentata*, Cuvier).

Dentes nulli; aut radices, cervicem, et adamantum carentes. Ungues, falculæ magnæ, plerumque vaginatæ, deflectentes.

TRIBUS—PHYLLOPHAGA.

Dentes pauci, e dentino vasculoso, dentino duro et cæmento compositi, dentino vasculoso axem magnum formante.

Apophysis descendens in osse jugali. Acromion cum processu coracoideo-concretum.

FAMILIA 1.—Tardigrada. (Syn. *Scansoria, Bradypodidæ.*)

Pedes longi, graciles; antici plus minusve longiores: manibus di-vel tridactylis, podariis tridactylis; digitis obvolutis, falculatis.

Arcus zygomaticus apertus. Cauda brevissima.

GENUS 1.—Bradypus, Linn., Illig. (Syn. *Acheus*, F. Cuvier.)

GENUS 2.—Cholœpus, Illig. (Syn. *Bradypus*, F. Cuv.)

FAMILIA 2.—Gravigrada. (Syn. *Eradicatoria, Megatheriidæ.*)

Pedes breves, fortissimi, æquales aut subæquales: manibus penta-vel tetradactylis, podariis tetra-vel tridactylis; digitis externis 1 aut 2, muticis ad suffultionem gressumque idoneis, reliquis falculatis.

Arcus zygomaticus clausus. Claviculæ perfectæ. Cauda mediocris, crassa, fulciens.

GENUS 1.—Megalonyx, Jefferson, Cuv. (Syn. *Megatherium*, Desm., Fischer.)

Dentes $\frac{5\text{-}5\,?}{4\text{-}4}$ subelliptici, coronide mediâ excavati, marginibus prominulis.

Pedes antici longiores. Tibia et fibula discretæ. Calcaneum longum, compressum, altum. Falculæ magnæ, compressæ.

SPECIES.—Megalonyx, Jeffersoni, Cuv. (Syn. *Megatherium Jeffersoni*, Desm., Fischer; *Megalonyx laqueatus*, Harlan.*)

GENUS 2.—Megatherium Cuv. (Syn. *Bradypus*, Pander et D'Alton.)

Dentes $\frac{5\text{-}5}{4\text{-}4\,?}$ contigui, tetragoni, coronide transversim sulcatâ. Manus tetradactyli; podarii tridactyli, digitis duobus externis muticis. Falculæ magnæ, diversiformes; medii digiti maximæ, compressæ. Femur capite integro; tibia cum fibulâ utrâque extremitate concreta. Astragalus paginâ anticâ supra excavatâ. Calcaneum longum, crassum.

SPECIES.—Megatherium, Cuvieri, Desm. (Syn. *Bradypus giganteus*, Pander et D'Alton.)

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* The species is founded on fossils from Big-bone Cave, Tenessee, described in the Medical and Physical Researches, p. 319–331. The author does not prove the specific distinction of these remains from the *Megalonyx Jeffersoni* of Cuvier.

GENUS 3.—Mylodon, Owen. (*Megalonyx*, Harlan,* *Orycterotherium*, Harlan.)†

Dentes $\frac{5\text{-}5}{4\text{-}4}$ discreti, superiorum anticus subellipticus, e reliquis modice remotus; secundus ellipticus; reliqui trigoni paginâ internâ sulcatâ: inferiorum anticus ellipticus; penultimus tetragonus; ultimus maximus, bilobatus. Pedes æquales: manus pentadactyli; podarii tetradactyli; ûtrisque digitis duobus externis muticis, reliquis falculatis: falculæ magnæ, semiconicæ, inæquales. Caput femoris ligamento rotundo impressum: tibia et fibula discretæ: astragalus paginâ anticâ supra complanata: calcaneum longum, crassum.

SPECIES 1.—Mylodon Darwinii, O. Maxilla inferior symphyse longiore angustiore; molaris secundus subellipticus; ultimus bisulcatus, sulco interno angulari.

SPECIES 2.—Myl. Harlani, O. (*Megalonyx laqueatus*, Harlan, *Orycterotherium Missouriense*, Harlan.) Maxilla inferior symphyse breviore, latiore; molaris secundus subquadratus; ultimus trisulcatus, sulco interno bi-angulari.

SPECIES 3.—Myl. Robustus, O. Maxilla inferior symphyse breviore, latiore; molaris secundus subtrigonus; ultimus trisulcatus, sulco interno rotundato.

GENUS 4.—Scelidotherium, Owen. (Syn. *Megalonyx*, Lund.)‡

Dentes $\frac{5\text{-}5}{\text{-}4}$ aut contigui aut intervallis æqualibus discreti; superiores trigoni; anticus inferiorum trigonus, secundus et tertius subcompressus, paginâ externâ sulcatâ; ultimus maximus, bilobatus.

Caput femoris ligamento tereti impressum; tibia et fibula discretæ. Astragalus antice duabus excavationibus. Calcaneum longum, crassum. Falculæ magnæ, semiconicæ.

SPECIES—Scelidotherium Leptocephalum, O.
Scel. Cuvieri, O. (Syn. *Meg. Cuvieri*, Lund.)
Scel. Bucklandi, O. (Syn. *Meg. Bucklandi*, Lund.)
Scel. Minutum, O. (Syn. *Meg. Minutus*, Lund.)

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* The lower jaw described by Dr Harlan, loc. cit. p. 334–335. It is erroneously ascribed to the *Megalonyx laqueatus*.

† Proceedings of the American Philosophical Society, Vol. ii. No. 20, p. 109.

‡ I am in doubt whether the term *Platyonyx*, subsequently proposed by Dr Lund, be really intended to apply to the animals of the genus *Scelidotherium*, seeing that the breadth of their claw-bones is equalled by the height, and vastly exceeded by the length, of the same: it would be very descriptive of the broad ungual bones of the *Glyptodon*, and its congeners.

GENUS 5.—Cœlodon, Lund.

Dentes $\frac{4\text{-}4}{3\text{-}3}$.

GENUS 6.—Sphenodon, Lund.*

Description of an Improved Long Slide-Valve for Condensing Engines. By Mr JOHN MAXTON, F.R.S.S.A., Engineer, Leith. Communicated by the Royal Scottish Society of Arts. With a Plate.†

The advantages of this valve are,—that it may be used without a steam-chest, while it has all the advantages of a long slide-valve in shortening the passages to the cylinder, it works with much less friction than the common long slide-valve, the pressure being equalized, and is much less expensive, and easier upheld, than the packed valve.

In fig. 1, Plate VII., the piston is represented as descending in the cylinder, the vacuum being formed under the piston by the passage A through the valve towards the condenser, the steam being admitted above the piston by the induction passage B, the steam at the same time having access to the portion of the valve below at D, which tends to press the valve outwards from the cylinder, while the vacuum, acting on the inside of the valve E, draws it towards the cylinder, the valve being so proportioned that the vacuum must so far overbalance the steam, that the valve will keep close to the cylinder.

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* Both this genus and *Cœlodon*, Lund, are indicated rather than satisfactorily established. The teeth of the Sloth are first developed in the form of hollow obtuse cones, and do not assume the cylindrical form until worn down to the part which has acquired, in the progress of growth, the normal thickness; and this is afterwards maintained, without appreciable alteration, during the subsequent uninterrupted growth of the tooth. The compressed molars of the *Scelidotherium*, which doubtless follow the same law of development, would present in the young animal the form of hollow wedges; and such I suspect to be the nature of those teeth, which are figured by Dr Lund in the above-cited Danish Memoir, plate xvii. figs. 5–10, and on which he has founded his genus *Sphenodon*.*

† Read before the Royal Scottish Society of Arts, 13th March 1843.

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* Owen on the *Mylodon robustus*, p. 168.

In fig. 2, the piston is at half-stroke, ascending, and the position of the valve is reversed from that in fig. 1.

Fig. 3 is a front view and section of the valve.

JOHN MAXTON.

LEITH ENGINE-WORKS, *24th January* 1843.

General Considerations regarding the Palæontology of South America compared with that of Europe. By M. ALCIDE D'ORBIGNY.*

At the period when the gneiss system of rocks was being deposited beneath the hot oceans which must then have covered the globe, animal existence could not have been maintained; and accordingly, in these beds, no trace of any animals has ever been detected. We may even go further, and affirm that the earlier deposits of the Silurian system were still devoid of them, at least in the American seas. And, in fact, three-fourths of the predominating] phylladian rocks, representing the Silurian in the New World, are destitute of organic remains—the earliest having appeared only towards the termination of that period. At that epoch, the Silurian ocean presented, in the southern hemisphere, an immense surface, whereupon there existed, as in Europe, different species of *Lingula*, *Orthis*, *Calymene*, and *Asaphus*, resembling in form those of the Old World, and identical with them. The uniform distribution of the species of this formation over all latitudes, from the torrid zone to the frozen regions of Russia, denotes a heat over the surface of the globe, so strong, that it extinguished the difference of temperature which is now referable to the difference of latitude.

Causes undoubtedly effecting new dislocations of the earth's crust, annihilated all the animals of the Silurian system; and the strata which overwhelmed them were themselves covered with new deposits. To the muddy sands of the American Silurian strata, quartz sands succeeded. A distinct fauna arose in the midst of the Devonian seas; and this fauna, consisting of *Terebratulæ*, *Spirifers*, and *Orthes*, exhibits features analogous to that of the animals of the European seas of the same geological epoch. But this group of animals was annihilated in its turn, and the Devonian fauna disappeared from the surface of the globe, after having existed for a considerable time, if we may venture to judge from the large proportions of its beds.

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* These CONSIDERATIONS are extracted from the Paleontological portion, not yet published, of M. D'Orbigny's *Voyage dans l'Amérique méridionale.*

To the Devonian system succeeded in America, as in Europe, the great series of the carboniferous strata. Then appeared a very varied marine fauna, in which, among the genera *Solarium*, *Natica*, *Pecten*, *Trigonia*, *Terebratula*, *Orthis*, and *Spirifer*, that of *Productus* shewed itself more numerous and more peculiar to this formation than the others. Compared with those of Europe, these American species exhibit, not only the greatest analogy, but also identical species, indicating a complete contemporaneity of existence. During the epoch of this carboniferous system, as much as during the Silurian, no difference of temperature was attributable to latitude, inasmuch as the same beings lived simultaneously under the torrid zone and throughout the Polar seas. The central heat was still immense, and continued to neutralize all external influence.

After a protracted continuance of the rich fauna of the carboniferous system, and of its still richer flora, when numerous palms and ferns adorned the Continent, whilst the countless products of its maritime fauna peopled the seas, nature again destroyed its own work. The animal and vegetable world was buried under rocky beds by the appearance of the *Chiquitian* system, and the *Triassic* period commenced. If in America, as in Europe, the beds which were deposited, form on the one Continent, as on the other, identical slate-clays and variegated sandstones, it is at least a curious circumstance, that in the New World we encounter only beds which are destitute of those numerous beings which exist in the strata of the Old. In explanation, it would be necessary to suppose, that during that period the Triassic seas of America were somehow in less favourable circumstances; possibly at the bottom of some maritime basin, where no animals could exist.

Posterior to the Triassic system, there commences in Europe a very long period, comprehending the Jura system, in which six formations, at least, often of great extent, succeed, all exhibiting distinct faunas; these are the Lias, the lower Oolite, the great Oolite, the Oxford, Kimmeridge, and Portland deposits. We in vain seek for these extensive formations in America, and with difficulty discover a trace of them in a small isolated locality. The question then very readily occurs, What has been the cause of the non-appearance of the Jura system? This, at first announcement, is a startling question. But if, for instance, we suppose that during this long period, the Triassic system, forming perhaps a prolongation of the Bolivian, and destined subsequently to support the Cretaceous system, was emerged, and formed Continents, we recognise a cause which may explain the want of the Jura system in the New World. It would appear probable, that, during the Jura formation, South America constituted a much larger Continent than at present; whilst, at the same time, for the explication of existing facts, it is likewise necessary to suppose that, at the conclusion of the Jura deposits, numerous dislocations occurred in America from the sinking and immersion of these same Triassic formations, because they then received the lowest formations of the succeeding epoch.

The cretaceous formations now appeared on the crust of the globe; and Nature, after having annihilated the previous fauna, repeopled it with animals, and animal life reappeared on the earth. Whilst, in Europe, numerous Ammonites and other Mollusca peopled the ancient seas of the Paris basin and the Mediterranean, belonging to the Neocomian period, these same seas extended to the northern and western coasts of America, from Columbia to the Straits of Magellan, exhibiting species approximating in form, and even identical in kind. In fact, not only do the Neocomian formations of Columbia afford fifty per cent. of species related in form to those of the Paris basin of this system, but no fewer than twenty per cent. of species quite identical are found simultaneously in Europe and in America.

On the contrary, the Neocomian formation of the Straits of Magellan exhibits analogies with the basin of the Mediterranean. However this may be, the Neocomian seas, with their molluscous animals, similar and identical, extended at the same time in the southern hemisphere, as far as the 54°, and in the northern from the 4° to the 48° of latitude, that is to say, to more than 2500 leagues in length, with a breadth of 75 degrees, or more than 1800 leagues. The laws which direct the actual geographical distribution of living beings upon the surface of our planet, always depend upon a complete uniformity of the conditions of existence and of temperature. Hence we ought, by comparison, to conclude that the simultaneous presence of the same species in the depths of the Neocomian seas of Columbia, of the Straits of Magellan, and of France, denote for this epoch, an unity of temperature throughout the different localities, which no longer exists at the present time, since Columbia is under the torrid zone, France is relatively a temperate country, and the region of the Straits of Magellan a very cold one. As regards the Silurian and Devonian systems, I have already directed attention to the agency of the central terrestrial heat which probably existed in those early periods in which living beings existed in the world. I have also dwelt upon the same agency, as operating at a later period upon the Carboniferous system. The attention I have directed to the Jurassic deposits of Europe has not less satisfied me, by the existence of the Oxford beds, in every way identical, in France and to the north of the Oural Mountains,* that the polar cold did not exist for about a half at least of the Jura period. To this I may now add, that I have reached the same conclusions with respect to the newer cretaceous formations. It appears then certain, that, during the Neocomian formation, not only was the terrestrial heat sufficiently strong to counteract the influence of the latitude of the temperate regions, but also completely to annihilate the icy influence of the poles.

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* I have arrived at this curious result by the comparison of the rich fossil remains which MM. Murchison and De Verneuil have discovered in the Jura formation of Russia, and which they have entrusted to me for examination and publication.

The Neocomian formations are replaced in Europe by the Gault. This member of the cretaceous formation,* so much subdivided, appears to be quite wanting in America; which is not the case with the chloritic chalks, as a portion of them is found in the Cordilleras of Chili. But then, as I have satisfied myself by comparisons, the Faunas, far from covering immense portions of the earth's surface, appear to be limited in their distribution. They are divided more and more up to the termination of the cretaceous system, which is marked in America by the first *relief* of the Chilian system of the Cordilleras,† and by the Gauranian deposit,‡ which is its immediate result.

Nature, in fact, ceasing for a time to be in repose, the retreat of the materials again induced vast depressions in the west, whilst a line of dislocation, fifty degrees in length, gives origin to the eastern Cordilleras, therewith producing—as the result of the balancing of the waters upon the then emerged continents, and at the bottom of the maritime basins of America—ferruginous beds, which do not contain any trace of organized bodies. This is the commencement of the tertiary period, an epoch when mammals were yet unknown. A calm then prevailed, and the New World exhibited maritime basins and circumscribed continents. After this there appeared, for the first time, numerous mammalia in the midst of a vigorous vegetation, and the sea was inhabited by marine animals much more diversified than formerly in their forms, but more restricted in their faunas. The same species were no longer to be found from one side of the globe to the other; and the uniform temperature due to the central heat having lost much of its intensity, the prevailing races of beings were more circumscribed, and composed, even under the same latitude, and at very inconsiderable distances from each other, local faunas, which were often distinct. This, at all events, is the state of matters which is found in the tertiary seas of South America, limited by a simple chain, that of the Cordilleras, which separates the fauna of the Pacific from that of the Atlantic. At the same time, a great number of marine animals are to be found on both sides of the Cordilleras, including various species of Bulla, Natica, Fusus, Rostellaria, Oliva, Venus, Cardium, Arca, Trigonia, and Perna; and the wood of the Coniferæ, and the bones of the Megamys and the Toxodon, were transported from the neighbouring continent into both seas.

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* See *Paléontologie Française (terrains Crétacés)*, tom i. p. 450 and 639.

† Géologie du Voyage dans l'Amérique meridionale, p. 272.

‡ Ib. page 245.

Judging from the extent of these deposits, matters continued for a very long time in this state in America; whilst in Europe we find the tertiary formation also very thick, as in the Paris basin, and there enveloping a great number of creatures, which constitute a distinct fauna, although, as in America, composed of species which are peculiar to hot climates. And if the seas continued for ages, during which the contained animals underwent no change, the neighbouring continents were not less favoured. During this period, there existed in the New World, along with a vegetation which was admirably fitted to support them, such mammalia as the Mastodons, the Megatherium, Megalonyx, Toxodons, and many other terrestrial animals different from the fauna which had preceded, and not less so from that which now exists. And the same phenomenon is exhibited in Europe: the Mastodons, Tapirs, Rhinoceroses, Elephants, and all the great animals which are now unknown, then inhabited our temperate and even cold regions. The world generally, although it did not support more of the same identical animal forms, did not the less present, and that everywhere, conditions as favourable for supporting animal life, along with a uniform distribution of beings which resembled each other in their large size, and in the requirements of their existence.

In the midst of the apparent repose of this active animalization of the land and the waters, a fresh catastrophe occurred. A new and considerable movement happened in the Chilian system. The Cordilleras acquired great additional elevation by the upraising and emergence, at the moment that the trachytic rocks appeared, of the bottom of the tertiary seas of the Pampas and the western coast. Not only would it appear that the marine fauna was annihilated, but the impulse given to the waters of the ocean, moreover, invaded the continents, sweeping all the animals along with them, depositing therewith terrestrial debris at all heights, in the terrestrial basins, and especially in the immense depression of the Pampas, which thus became the great sepulchre of the terrestrial fauna. It was there also that the bones, or the entire animals themselves, when they were not floated away, were thrown into the clefts of the rocks, or into the deep caverns of Brazil. Were we to enquire what occurred in Europe at the same epoch, we might with probability associate with it the disappearance of the Elephants, Tapirs, Rhinoceroses, Mastodons, and other terrestrial animals of extinct races, which are found in the mud of La Bresse (analogous to that of the Pampas), under the trachytic conglomerates of Auvergne, and those which later causes have brought to the surface of the European soil. Such being the case, the compound faunas of the great animals of the extinct races must have simultaneously inhabited the old and new world; and their destruction in both these great localities must have arisen from the same cause, namely, the action of one of the upraisings of the Cordilleras.

After this catastrophe, the globe probably remained for a long time inactive, before the great Almighty Power again clothed it afresh with the vegetable and animal existences which cover it at present, crowning his wonderful work by forming man, and making him lord of this lower world. At all events, it appears clear that if there have since been any partial movements at the surface of the globe, none has been sufficiently powerful to destroy the prevailing fauna. The tradition of a deluge, which is preserved by every nation under heaven, from the most civilized in Europe to the still half savage American, whether in the forests or on the heights of the Cordilleras, seems to be nothing more than the faint recollections of those general causes which, by the outbreak of vast volcanos, induced the last great changes on the surface of our globe. In America, at all events, these changes are most marked; and we must attribute to them the transport above the present level of the ocean, of the fossil shells upon both the eastern and western sides of South America, and especially of the shells of the Pampas, which consist of none others than those species which are at present inhabiting the neighbouring seas. It is to this movement also that we must attribute those modern submersions of different extents, whose manifest traces are every where to be found upon the diluvian formations of the new world. In the old world, also, they also shew themselves in innumerable localities. Of this the oyster-beds of St. Michel-en-l'Herm are a proof, as likewise those changes in the level of the modern beds of the quaternary system of the north of Europe. Thus, both in America and in Europe, in the later effects, as in the earlier, we perceive a great coincidence, both in the causes and in the results.

CONCLUSIONS.

From the comparison of the Palæontological facts which have been observed in the New World and in Europe, we may deduce conclusions of immense importance for the solution of the great general questions of geology, and of the chronological history of the animated worlds at the surface of the globe. The conclusions are as follows:—

1. Living beings, taken as a whole, have, according to the chronological order of the faunas peculiar to the formations, advanced, both in America and in Europe, from the more simple to the more compound. Many genera, it is true, like the Trilobites, Orthoceratites, and Producti, have completely disappeared with the more ancient systems; others, appearing at a later period, the Ammonites, Belemnites, Turrilites, &c., &c., have also become extinct with the completion of the cretaceous formations; but the genera, multiplying more and more in proportion as they receded from the primeval ages of the world, have been replaced, during the tertiary period, by Mammalia more perfect in their organization, and by marine and terrestrial animal forms which were previously unknown, and many of which are represented in the existing fauna.

2. As no transition can be detected in the specific forms, the living beings at the surface of the globe appear to have succeeded each other, not by transition from one into another, but by the extinction of the existing races, and by the new creation of species at each geological epoch.

3. The animals are distributed by zones, according to the geological epochs. Each of those epochs, in fact, represents

at the surface of the globe, a fauna, which is distinct, but identical in its composition. Thus the Silurian, the Devonian, the Carboniferous, Triassic, Cretaceous, Tertiary, and Diluvian formations, are the same in America as in Europe, and preserve the same palæontological *facies*, the same generic forms.

4. Not only is there the same *facies* in the extinct fauna of the Old and New World, but some identical species common to both, moreover prove, that they were quite contemporaneous.

5. This contemporary existence which is observed at immense distances, in the early periods of animalization, and down to the epoch in which the lower cretaceous formations were deposited, appears to depend upon the prevalence of a uniform temperature, and on the shallowness of the oceans, which allowed the living beings not only to enjoy completely the influence of the external light, a condition quite indispensable to their existence, but also to propagate and spread without obstacle, from one locality to another,—circumstances which could no longer occur when, through the influence of inequality of temperature, the cooling of the earth on the one hand, and the elevation of the various systems on the other, together with the corresponding depression of the oceans, many insuperable barriers were presently occasioned to their spreading either along the coasts, or in extensive sedimentary deposits. It is then apparent, that the uniformity of the distribution of the first living beings which flourished upon the globe, was owing as much to the equality of temperature produced by the central heat, as to the shallowness of the seas; whilst the subdivision of faunas, as we approach the present period, by basins, which become more and more confined, arises from the refrigeration of the earth, from the existence of terrestrial and marine barriers, which presented obstacles to the extension of local faunas.

6. If faunas are found to have the same points of separation in the New and Old World, and if they agree in the same marked limits as to their palæontological composition, then we must naturally conclude, that the divisions of formations do not depend upon partial causes, but must have pro-

ceeded from general ones, whose influence has extended over the whole globe.

7. After the examination of the great geological facts of the New World, these general causes, I conceive, may easily be apprehended. In the last elevation of the Cordilleras, and in the distribution of the fauna which has resulted therefrom, we may from analogy conclude, that the annhilation of the faunas, partial or complete, peculiar to each period or formation, always results from the amount of the dislocations at the surface of the globe, caused by the retreat of matters resulting from the cooling down of the central parts of the earth, and from the disturbance which these dislocations produced. A system like that of the Andes, for example, 50° in length, of which we form our judgment from its height above the surface, without dwelling upon the corresponding extent of its sinking and depression in the depths of the ocean, must have produced such a commotion in the water, from the displacement of matter, that its effect must have been universal both over sea and land. The latter must have been ravaged by the removal of all terrestrial beings; and the former, by the transport of terrestrial debris, which must have suffocated not only the animals desporting in the wide ocean, but also those which were sedentary and on the coasts, by the deposit with which they must have been covered. This explains at once the separation of fossils into systems, and the separate extinction of these, at each great geological formation.

8. M. Elie de Beaumont has thrown out the sublime idea, that the conclusion of each geological period was invariably produced by the elevation of the different systems which furrow the surface of the globe. It will be observed, that the several palæontological results observed in the New and Old World, would entirely corroborate this opinion; that, moreover, the results of these dislocations being so general throughout the globe, and manifesting themselves at immense distances, we ought to inquire, in these systems, ancient or modern, for the causes of the annihilation of the numerous fauna which have succeeded each other on the surface of our planet. When in the neighbourhood of the localities in which we now find these distinct faunas, nothing can be found in the

systems explanatory of the phenomena, we must then seek for this explanation in more distant parts, at points still unknown to science; or suppose, that if these terrestrial systems have not been the real cause, some may have been destroyed by great subsidences and depressions. It is not to be forgotten, that the terrestrial systems constitute only the visible parts of the dislocations of the globe; whilst the depressed portion, often the greater of the two, being frequently covered up, is unknown to us, and may always continue so. In a word, the separation of distinct faunas by systems and formations, is only the visible result of the elevation and depression, to various extents, of the crust of the globe in all its parts.

9. I have already directed attention to the fact, that, from the uniform distribution of the same animals, it follows, that, until the commencement of the cretaceous formations, the central heat of the earth completely destroyed the influence of latitude, and of the polar cold. If at that time atmospheric influence had no effect upon the distribution of animals on the surface of the globe, all the faunas must necessarily have owed their circumscribed limits to great dislocations of the globe. It was only posterior to the cretaceous deposits, that the influences of latitude could have complicated the partition of basins, multiplied the local faunas, and destroyed that uniformity of distribution, which we still find in more ancient formations.*

On Sigillaria, Stigmaria, and Neuropteris.

In our next Number, we expect to publish a valuable paper on these fossils, by Mr King, the Curator of the Newcastle Museum, who for some time past has been more or less engaged in endeavouring to prove that they are different parts of one and the same plant.

The following is a summary of the principal conclusions contained in the paper.†

Owing to the bark of *Sigillaria* being composed of two different layers of tissue, it is shewn, that six different impressions may be produced;

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* From the *Annales des Scien. Natur.* Mai et Juin, 1843.

† A portion of the paper consists of an abstract of M. Adolphe Brongniart's "Observations sur la structure interieure du *Sigillaria elegans* comparée à celle des *Lepidodendron* et des *Stigmaria* et à celle des végétaux vivants."

this, together with the circumstances of one individual stem having its ribs wide at the base, as in *S. reniformis*, and narrow at the top, as in *S. pachyderma*, and the petiole scars in close contact, as in *Favularia tessellata* (Fossil Flora, pl. 75), it is maintained, has led to the establishment of a number of factitious species, and even genera.

Respecting the fluted character of *Sigillaria*, the opinion is advanced, that it is due to a peculiar arrangement of the leaves on the stem, or, in other words, that it arises from a slight modification of the law which produces the rude channellings on young branches of some Conifers, rather than to the cause which produces the more obvious furrows of certain Cactuses, if, as Brongniart appears to think, the furrows of the latter plants result from the corresponding character on their ligneous cylinder.

In working out the foliage of this plant, an examination is first made of the various leaves which have been found in the coal-measures. Of these, it is concluded, that *Pecopteris* and *Neuropteris*, the one or the other, from their abundance and other circumstances, possess the strongest claims to be considered as having belonged to *Sigillaria*. The inquiry is next entered upon as to which of these fossils is the one to be so correlated; an inquiry which, it will be evident, ought to be influenced, to a certain extent, by the discovery of Brongniart, that *Sigillaria* is allied both to the Ferns and to the Cycases, because, it seems to follow, that if the trunk shewed its situation to be intermediate to the last two tribes, so the leaves should possess the like character. Now, as *Pecopteris* is a normal fern, and *Neuropteris* an abnormal one, in some respects, and as the last bears the closest affinity to *Odontopteris*, a genus which gradually conducts us to the Cycadeous type, that is, of the Jurassic period—through certain species of *Otopteris*, the conclusion is drawn, that *Neuropteris* constituted the foliage of *Sigillaria*. This inference, it is maintained, is further supported by the relative geological ages of these foliaceous remains.

Respecting the root of *Sigillaria*, this is shewn to be *Stigmaria*.*

By an examination of the dome-shaped *specimen* figured in the preface to the 2d vol. of the Fossil Flora (P. xiii.), and preserved in the Newcastle Museum, the error is corrected, that *it* exhibits " the upper surface of the central portion" of a *Stigmaria*; on the contrary, it is shewn to be, at least the crown, merely the impression of the under surface of the fossil, in short, that it is nothing more than a convex mass of hardened mud answering to the under side or concavity of a root.

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* Mr King announced this opinion at a meeting of the Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne, on May 23, 1842 (*vide* Literary Gazette, 1842, p. 425); and in May 17, 1841, previously to examining all the evidence bearing upon the question, he read an account of a specimen of *Sigillaria*, belonging to the Newcastle Museum, and adverted at the same time to some characters occurring on the base of this fossil, " apparently leading to the conclusion, that *Stigmaria* is the root of *Sigillaria*."

From a notice in the Athenæum of August 20, p. 772, it appears that Mr King's opinion has been completely confirmed by the discovery of Mr Binney of an upright *Sigillaria*, nine feet high, in the coal strata at St Helen's, near Liverpool, having the roots remaining, which are the undoubted *Stigmaria ficoides*.

We will now proceed to give a brief view of the principal evidences which have led to the opinion that *Stigmaria* is a root, and that it belongs to *Sigillaria.*

First, the correspondency of the sinuous lines on the crown of the above dome-shaped specimen, with those observable on the under side of the root-stock of a *Sigillaria* in the Newcastle Museum; second, the correspondency of two *furrows* placed at right angles to each other, observable on the under side or *concavity* of the root stock on two specimens of *Sigillaria*, with the *ridges* seen on the crown or *convexity* of the dome-shaped specimen; third, the root-like form of perfect specimens of *Stigmaria*; fourth, the position of the external appendages of the last specimens, which is such, that they must have penetrated the now consolidated mud in which they are imbedded, as shewn by Steinhauer and Hutton: fifth, the resemblance of the furrows on the origins of the branches of *Stigmaria* with those on the base of *Sigillaria*, thereby shewing the one to be a continuation of the other; sixth, the similarity between the internal structure of *Sigillaria* and *Stigmaria*.*

Some points connected with the internal structure of *Stigmaria* are alluded to, and it is shewn that the large intercellular spaces which characterize the ligneous cylinder of this plant, are not true "great medullary rays," as has been generally supposed, but openings which served for the passing out of the cords from the vascular system into the external appendages, and analogous to what is observable in *Anabathra* and *Lepidodendron.* Some additional information as to the internal structure of these latter fossils is given as derived from the examination of a number of sections.

Certain objections which have been urged against *Stigmaria* being a root, are next considered,—especially the spiral arrangement of the appendages, and the presence of a pith; but the force of those objections, it is contended, is considerably diminished if we look upon this fossil as combining the form of a true *Radix* with the characters of a *Rhizoma*,—at least in such of the latter as exhibit, in the regular arrangement of their fibrils, and in their anatomy, but little difference from what obtains in the stem.

Respecting the situation of *Sigillaria* in the vegetable kingdom, although its cycadeous character is fully admitted with Brongniart, the probability of its being related to the Angiospermous Dicotyledons as supposed by this learned botanist, is shewn to be somewhat remote:

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* From this similarity, M. Brongniart was led to suppose that *Stigmaria* would turn out to be the root of *Sigillaria.*

the presumptive evidence in favour of *Neuropteris* having been its foliage, it is maintained, so far invalidates this supposition, and the internal structure is considered to involve a powerful argument against it, inasmuch as the medullary sheath of *S. elegans*, appears to be the analogue of the regularly-netted cylinder of vascular tissue of some ferns (*e. g. Aspidium Filix-mas*). Accordingly, it is argued that the stem of this species has the radially arranged ligneous zone of the Cycases superadded to the vascular net-work of certain Ferns. The genus *Sigillaria* is therefore concluded to be essentially intermediate to the Vascular Cryptogams and the Cycadeous Gymnosperms.

On the Chemical Constitution of Philipsite or Lime Harmotome. By Arthur Connell, Esq., F.R.S.E., Professor of Chemistry in the United College and University of St Andrews. Communicated by the Author.

The late researches of Köhler* appear to be sufficient to shew that the constitution of the barytic and lime harmotome cannot be expressed by the same chemical formula, on substituting lime for barytes. This conclusion follows principally from his analysis of the barytic species, because that examination shews a complete conformity in the results of the analysis of that species from three different localities; whereas the previous analyses differed materially from one another; my own, of the barytic harmotome of Strontian† being the only one in nearly perfect accordance with Köhler's result. We may therefore probably take the formula proposed by Köhler, $2\left.\begin{matrix}\mathrm{B}\\ \mathrm{K}\end{matrix}\right\}\mathrm{S}^4 + 7\,\mathrm{A}\,\mathrm{S}^2 + 12\,\mathrm{Aq}.$ as representing the constitution of the barytic mineral.

The exact constitution of the lime species is, however, not so clear. The analysis of the lime harmotome of Marburg and Kassel, given by Köhler, differs considerably from those published by Gmelin and Hepel,‡ of this mineral, from the former of these localities; the oxygen of the one-atomed bases, according to the first of these chemists, being 2.56, and, according to the others, in one instance, 2.88, and in another,

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* Poggend. Annal., xxxvii. 561.

† Ed. New Phil. Jour., xiii. 33.

‡ Annales de Mines, x. 355.

3.13,—differences which, as the amount of the other constituents is much the same in all the cases, render it scarcely possible to represent all the results by the same formula.

It therefore appears to me that this part of the subject affords room for farther investigation; and as a contribution to that end, an analysis of the Philipsite or lime-harmotome of the Giant's Causeway will perhaps not be unacceptable.

The specimen examined was in well-sized white and translucent twin crystals of the ordinary harmotome form. Their sp. gr. was found to be 2.17, at 61°.

A portion of the crystals lost by ignition 16.96 per cent. of water.

20 grains in fine powder were treated with strong muriatic acid, when immediate gelatinization took place; and, by the usual steps of analysis, there were obtained,—

Silica, . .	9.47
Alumina, . .	4.36
Lime, . .	0.97
Potash, . .	1.11
Soda, . . .	0.74
	16.65

or in 100 parts,—

			Oxygen.	
Silica, . .	47.35		24.83	8
Alumina, . .	21.80		10.18	3
Lime, . . .	4.85	1.36		
Potash, . .	5.55	.94	3.24	1
Soda, . . .	3.7	.94		
Water, . .	16.96		15.07	4½
	100.21			

This would give the formula $2\left.\begin{matrix}C\\K\end{matrix}\right\}S^2 + 6\,AS^2 + 9\,Aq.$

This result differs materially in regard to the amount of the one-atomed bases from those of Köhler, and approaches more nearly to those of Gmelin and Hepel, particularly to one of their results; the proportion of the oxygen in which instance, will admit of being expressed by the above numbers, almost as well as by any others, or at least will thus yield the most admissible formula. These chemists found in the case alluded to—

			Oxygen.
Silica,	48.02		25.03
Alumina,	22.60		10.3
Lime,	6.56	1.84	3.13
Potash,	7.50	1.27	
Protox. of iron,	.18	.02	
Water,	16.75		14.88
	100.61		

Considering, however, that the other analyses of Philipsite, by Köhler and Gmelin and Hepel, can hardly be brought within this formula, I should by no means be inclined to give it as one which may be adopted with perfect certainty.

The presence of soda which has not been previously observed in this mineral, was clearly ascertained, as it also was in the previous instance of the barytic harmotome of Strontian, although in much less quantity. Indeed, I have reason to believe, that the presence of a portion of this alkali in minerals, usually regarded as containing potash only, is much more frequent than is commonly supposed.

If we assume what it would seem can now hardly be doubted, that there is an essential atomic difference in the chemical constitution of the two species, we shall of course be prepared to expect that the measurements of the angles will also present differences, although the general form of the crystals may be similar in the two cases; and the ordinary opinion of crystallographers appears to be in favour of the existence of such differences.

Proceedings of the Royal Scottish Society of Arts.

(Continued from Vol. xxxiii., p. 414.)

14*th March* 1842.—Sir John Robison, K.H., President, in the Chair. The following Communications were made:—

1. Part Third of Mr George Glover's communications on Drowning, and observations on the best means of Resuscitation, being the principles of Pneumatics applied with a view to improve the apparatus employed by Humane Societies for producing Artificial Respiration, and an investigation of the Pneumatic Laws involved in the operation. Thanks voted. (956.)

Mr Glover was requested to furnish an Abstract of Practical Rules and Directions for Saving from Drowning, and restoring Suspended Animation.

2. Burden's American Shingling Machine, used instead of the Tilt-Hammer, for squeezing the blooms, or preparing iron for rolling, was described by Laurence Hill, Esq. C. E., Glasgow, F.R.S.S.A. A working Model and Drawing were exhibited and presented to the Society. Thanks voted. (851.)

3. Description of a new Protracting Table for laying down extensive surveys, constructed on the principles of the one for which he obtained a premium from the Society some years ago. By George Buchanan, Esq. F.R.S.E., F.R.S.S.A. civil engineer, Edinburgh. The Table was exhibited. Thanks voted. To be printed in the Transactions. (875.)

4. Description, with a Drawing, of a Boring Machine for boring Artesian Wells. By Mr Robert S. Burn, 4 Tobago Street, Edinburgh. Referred to a committee. (832.)

5. Notice of an improved Rain-Gauge. By Thomas Stevenson, Esq. civil engineer, Edinburgh. Thanks voted. To be printed in the Transactions (837.)

PRIVATE BUSINESS.

I. The following Candidates were elected Fellows, viz. :—

1. Hamilton Pyper, Esq. advocate, Royal Crescent.
2. Archibald Dymock, M.D. 19 Pitt Street.
3. John Richardson, Esq. W.S. 21 Pitt Street.
4. John Murray, Esq. 24 Ainslie Place.

28th March 1842.—John Shank More, Esq. F.R.S.E., Vice-President, in the Chair.

The following Communications were then made :—

1. The Lime Light was exhibited, and its application to the Oxyhydrogen Microscope shewn by various arrangements of Lenses. By Mr Thomas Davidson, F.R.S.S.A., Optician, Edinburgh. Thanks voted. (892.)

2. The Report of the Committee on Mr Yule's Expanding Screw-Tap, Drill, and Mandrill, was read and approved of. Mr Milne, Convener. The tools were exhibited, and also Mr Hick's Expanding Mandrill. (820.)

3. Brockedon's Patent Stoppers for using in place of Corks were exhibited by Mr James Dowie, bootmaker, London and Edinburgh, F. R. S. S. A. Thanks voted. (890.)

4. On the comparative Evaporative Power of Coal and Coke. By Andrew Fyfe, M.D., F.R.S.E., F.R.S.S.A. Thanks voted, and ordered to be printed in the Transactions. (871.)

5. Description of a Self-acting Feeder for Steam-Boilers. By Mr Andrew Carrick, 107 Buchanan Street, Glasgow. A model was exhibited. Referred to a Committee. (859.)

6. A Box with Types so formed as to enable the Blind to commit their thoughts to Writing, capable of being easily read either by those who see or

those who are blind, was exhibited. By John Alston, Esq. of Rosemount, Glasgow, Hon. M.R.S.S.A. Thanks voted. (860.)

PRIVATE BUSINESS.

I. The following Candidates were balloted for, and elected as Fellows, viz :—

1. Mr John Neil, brassfounder and gasfitter, 10 George Street.
2. James Denham, Esq. Assistant-Clerk of Session, 3 West Lauriston Lane.

11*th April* 1842.—John Shank More, Esq., F.R.S.E., Vice-President, in the Chair. The following Communications were made :—

1. On the Action of Water on Lead. By Professor Christison, F.R.S.E.

2. An experimental Electro-dynamic Apparatus. By George Glover, Esq., F.R.S.S.A., Lecturer on Natural Philosophy, Edinburgh. The Apparatus was exhibited in action. Thanks voted. (891.)

3. Mr George Tait, advocate, F.R.S.S.A., described the manner in which the effect of fog coming on or going off is produced in a Portable Diorama constructed by him, which was exhibited to the Society in November last. The Diorama was exhibited in operation. Thanks voted, and abstract to be printed. (894.)

4. Description, with a Drawing, of a Self-acting Damper for regulating the draught of Furnaces. By Mr Robert S. Burn, engineer, 4 Tobago Street. Edinburgh. Referred to a Committee. (886.)

5. Suggestions, with a Drawing, of an Enclosed Gas Lustre, calculated for better ventilation. By the Rev. James Brodie of Monimail, Associate R.S.S. Arts. Referred to a Committee. (883.)

6. The Committee on making the arrangements for the proposed Exposition of Electro-Magnetism by Dr Fyfe, for the benefit of Mr Davidson, 9 St Andrew Square, reported. Captain Donaldson Boswall, Convener.

25*th April* 1842.—John Shank More, Esq., Vice-President, in the Chair.

After the minutes were read and approved of, the thanks of the Society were, on the motion of James L'Amy, Esq., unanimously voted to Dr Fyfe, for the excellent exposition of Electro-Magnetism, lately given by him at the request of the Society; which were given to Dr Fyfe from the Chair. . The following Communications were then made :—

1. On the Consumption of Smoke and Economy of Fuel by the use of Steam, in the patent process of Ivison. By Andrew Fyfe, M.D., F.R.S.E., F.R.S.S.A.

2. Description, with Drawing, of a new Water-wheel. By Mr James Wight, Manager of the Water of Leith Mills. A working Model was ex-

hibited. In this wheel the buckets are not fixed to the periphery of the wheel, but hang loosely on it as a chain, and thus retain the water longer, and consequently are said to give more power. Referred to a Committee. (878.)

3. Description of a Joiner's Cramp for laying flooring. Invented by Mr John Liddell, Ravensworth Castle, Yorkshire. Communicated by Mr James Milne, F.R.S.S.A. The tool was exhibited and presented to the Society, and its method of operation shewn. Referred to a Committee. (901.)

4. Letter from Sir J. Robison, K.H., President R.S.S. Arts, relative to the Disc Engine, Button Shank Machine, Tessalæ and Tiles, Apparatus for Distilling Sea-Water, and Electro-Magnetic Power. Thanks voted. (902.)

9th May 1842.—Sir John Robison, K.H., President, in the Chair. The following communications were made:—

1. On Stone and Wooden Pavements. By Mr D. T. Hope, F.R.S.S.A., engineer, Liverpool. Models and Drawings were exhibited. Referred to a Committee. (862.)

2. Description of "Venetian Pavement" for Halls, Conservatories, &c., made in Edinburgh under the direction of C. H. Wilson, Esq., A.R.S.A., F.R.S.S.A., architect, Edinburgh. Two specimens were exhibited. Referred to a Committee. (893.)

This flooring is formed of a mixture of Roman cement (in place of Pozzolano, which is used in Italy), and lime, with small pieces of broken marble of different colours, strewed over the mass while still wet. The marble is beat down to a level, and after the mass has become dry, the surface is brought to a beautiful polish with oil, very much resembling granite. The colour depends very much on the kinds of marble employed, and the whole forms a hard and solid floor, three or four inches thick, without crack or seam. The expense, it was stated, would be nearly the same as Arbroath pavement.

3. A Paddle-Wheel of a new construction in regard to the distribution of the Floats. By Mr Andrew Carrick, 107 Buchanan Street, Glasgow. A model was exhibited. Referred to a Committee. (858.)

The Reports of the following Committees were read and approved of, viz.:—

1. On Mr Yule's Compensation Governor. Mr Slight, Convener. (819.)
2. On Mr M. Paterson's Scotch Ultramarine. Mr Wilson, Convener. (828.)
3. On Mr Burn's Boring Machine. Mr Slight, Convener. (832.)
4. On Rev. J. Brodie's suggestions for an enclosed Gas Burner. Mr Milne, Convener. (883.)

13th June 1842.—Sir John Robison, K.H., President, in the Chair. The following Communications were made:—

1. An apparatus to be occasionally attached to a Turning-Lathe with a short slide, to fit it for cutting Screws of the whole length of the bed of the Lathe, was exhibited by Sir John Robison, K.H., F.R.S.E., P.R.S.S.A. Thanks voted to Sir John, who was requested to give a written description to be printed. Referred to a Committee. $\left(\frac{905}{a}\right)$

Sir John Robison submitted to the meeting this apparatus, which being attached to a slide-rest lathe, fitted it for cutting screws of a considerable variety of pitches, and of any length the lathe could take in.

The apparatus differed from the usual constructions in having only four pairs of toothed wheels, and in only one pair being used at a time, thus doing away the risk of error by the introduction of wrong intermediates. Another variation from the ordinary constructions appeared in the mode of connecting the leading screw of the slide-rest with the wheel-work, which was in this case effected by a square rod sliding in a tube, and adjustable at any required length; the connection was terminated at each end by an universal joint, by means of which the cutter-holder of the rest was free to advance and retreat sufficiently for executing the work.

Sir John explained, that, by limiting the number of change-wheels to four pairs, a considerable simplification is obtained; while, as the four pairs afford seven permutations (as either wheel of each pair may be made the driver) a sufficient variety of pitches for all the ordinary wants of the workshop may be obtained by a few changes of leading screws in the apparatus on the table; the addition of a leading screw of 10 turns, and one of 36 turns per inch to the ordinary screw of the rest (which has 12 turns) gives 21 pitches, varying from 3½ turns to 108 turns per inch—and these pitches may be either of right or left hand threads.

2. Sir John Robison also exhibited specimens of Sheet-Glass (white and coloured), perfectly flat, and without cockle or wrinkles; from Messrs Chance's glass-works, Birmingham. Thanks voted. $\left(\frac{905}{b}\right)$

3. A Working Model of his Patent (Piston) Rotary Steam-Engine was exhibited in action. By Charles Cameron, Esq. F.R.S.S.A. Thanks voted. $\left(\frac{905}{d}\right)$

4. Specimens of a Bituminous Substance found in crevices where a Dike occurs in the sandstone of Binny Quarry, and of Candles made from it, were exhibited. By Mr William Hogg, quarrier, Uphall. Thanks voted. $\left(\frac{509}{e}\right)$

5. Sir John Robison exhibited a Model of Wood Paving, now being tried in Paris, made from small trees. Thanks voted. $\left(\frac{905}{c}\right)$

The following Reports of Committees were read and approved of, viz.—

1. On Mr Meikle's Muted Clarionet. Mr R. Hunter, Convener. (844.)

2 On Mr Maxton's Stopper for Winding-Engines. Mr D. Stevenson, Convener. (835.)

3. On Mr Robertson's Signals for Railways. Mr Slight, Convener. (849.)

4. On Mr Carrick's Self-acting Feeder for Boilers. Mr R. Hunter, Convener. (859.)

5. On Mr Burn's Self-acting Damper. Mr Steele, Convener. (886.)

6. On Mr Wight's new Water-Wheel. Mr Glover, Convener. (878.)

7. On Mr Hope's Stone and Wooden Pavement. Mr R. Hunter, Convener. (362.)

The following Reports of Committees not being ready, were ordered to be lodged with the Secretary in the course of a month, and to be laid by him before the Prize Committee, viz.:—

1. On Mr Thomson's Tilting Apparatus. Mr D. Stevenson, Convener. (840.)

2. On Mr Davidson's improvements on the Solar and Oxyhydrogen Microscope, &c. Mr D. Stevenson, Convener. (857.)

3. On Mr Liddel's Joiner's Flooring Cramp. Mr Black, Convener. (901.)

4. On Mr Wilson's Imitation of Venetian Pavement. Mr R. Brown, Convener. (893.)

5. On Mr Carrick's Paddle-Wheel. Mr Glover, Convener. (858.)

List of Prizes of the Royal Scottish Society of Arts for Session 1843–44.

The ROYAL SCOTTISH SOCIETY OF ARTS proposes to award Honorary Medals, and Pecuniary Prizes, for approved Communications.

No Prize to exceed Thirty Sovereigns.

The attention of the Fellows and of the Public is directed to Inventions, Discoveries, and Improvements in the *Mechanical* and *Chemical* Arts in general, and also to means by which the *Natural Productions* of the Country may be made more available; and in particular to,—

1. INVENTIONS, Discoveries, and New Processes in the Useful Arts.
2. INVENTIONS, Processes, or Practices from Foreign Countries, not generally known or adopted in Great Britian.
3. NOTICES of Processes in the Useful Arts actually practised in this Country, but not generally known.
4. EXPERIMENTS applicable to the Useful Arts.

5. **Practical Details** of Public or other undertakings of National importance,—not previously published.

6. **Methods** of Economising Fuel, Gas, &c.—of preventing Smoke and Noxious Vapours from Manufactories,—of Ventilating Public Edifices, Private Dwellings, Ships, &c.—of constructing Buildings on the most correct Acoustic principles.

7. **Improvements** in the Manufacture of Iron, and other Metals, simple or alloyed; in the making and Tempering Steel, —in Ornamental Metallic Casting—in Calotype, Daguerreotype, and Electrotype,—in the preparation of Lime and Plaster for Fresco Painting, and in appropriate Tools for laying the Plaster with precision,—in Electric, Voltaic, and Magnetic apparatus,—in Pavements,—in Balance or Pendulum Time-keepers,—in Taps and Dies,—in Die-sinking,—in Wood-cutting,—in Printing from Wood-cuts,—in Printing-Presses,—in Stereotyping, and in cleaning the plaster from the Types,—in Furnaces and other apparatus used in Stereotyping,—in Type-Founding,—in the Composition of Printers' Rollers,—in Engraving on Stone,—in Ship-building, with regard to Ventilation, both for the crew and the timbers,—in Currying and Tawing of Leather,—in Glass and Porcelain, —in Land, Marine, and Locomotive Steam-Engines and Railway Carriages, and particularly in their Axles,—in Railway Telegraphs and Signals,—in Smith-work and Carpentry—in Tools, Implements, and Apparatus for the various Trades.

KEITH PRIZE.

The **Society** also proposes to award the **Keith Prize**, value **Thirty** Sovereigns, for an important Invention, Discovery, or Improvement in the Useful Arts.

General Observations.—The **Keith Prize** may be awarded for any communication which shall comply with the terms of the announcement of that Prize, although falling under any of the above specified subjects.

The Descriptions of the various inventions, &c. to be *full* and *distinct*, and, when necessary, accompanied by *Specimens*, *Drawings*, or *Models*.

The Society shall be at liberty to publish in their Transactions copies or abstracts of all Papers submitted to them. All Models, Drawings, &c. for which Prizes shall be given, shall be held to be the property of the Society,—the Society being in the practice of taking the value of the Model into account in fixing the amount of the Prize.

All Communications must be written on *Foolscap* paper, leaving margins at least one inch broad, on *both the outer and inner sides of the page*, so as to allow of their being bound up in volumes; and all drawings must be on *Imperial* Drawing Paper, unless a larger sheet be requisite.

Communications, Models, &c. to be addressed to JAMES TOD, Esq. the SECRETARY, 21 Dublin Street, Edinburgh, Postage or Carriage paid; and should be lodged *on or before* 1st *November* 1843, *or as soon thereafter as possible*, in order to ensure their being read and reported on during the Session.

Copies of this List of Prizes may be had from the Secretary.

EDINBURGH, 24*th April* 1843.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Observations on the Aurora Borealis, made at Kaafjord in Norway.*—Mr Ihle, an officer of the English Copper-mine Company at Kaafjord (69° 58′ 15″ N. Lat.; 23° 43′ 10″ E. Long.), near Alten, in Finmark, Norway, collected a large mass of meteorological observations, between January 1839 and the end of July 1841. Mr Reich has communicated to Poggendorff's Annals (1843, No. 2, p. 337) a portion of the results of M. Ihle's observations on temperature, and on the mean height of the barometer; and likewise interesting notices of the Aurora Borealis. Regarding the sound emitted by this meteor, we give the following extracts from M. Ihle's Meteorological Journal:—1840, *January 28th, Evening.* There was *distinctly* heard *a noise* corresponding to the movement of the rays of light, and resembling the rustling of silk stuffs. The stillness which prevailed in the air at the time, and which was of rare occurrence, left no doubt about the matter.—1840, *March 22d, Evening.* Calm and serene; northern lights in thin fine stripes of light across the heavens from W. to E. In the W. and S.W., very bright, peculiar-looking yellowish green luminous clouds proceeded from the horizon upwards,

constantly changing in their form and in the intensity of their light. At the instants of the greatest development of light, viz., when the luminous cloud was near the zenith, *there was quite certainly a sound heard*, resembling the rustling of straw or of cloth made of silk.—1840, *Nov.* 21*st*, 5 *o'clock p. m.* Calm. The atmosphere had cleared up, and rays of light shot simultaneously from the west and east horizon with the greatest brilliancy; at the zenith they whirled themselves round in a circle, and formed crowns of light. *A soft rustling was at the same time audible.* Ihle makes the following general observations on the Aurora:—The northern lights for the most part make their appearance over the western and north-western, and over the eastern and north-eastern horizon, rarely in the due north, and still more rarely over the southern horizon. The form of the northern lights is sometimes in stripes, and that either with an uniform division of the light or in bands with parallel streaks; and sometimes in bundles of rays, either ranged in certain lines next one another, or separated into undefined groups. The boundary of the stripes of light is sometimes sharp, sometimes obscure. More rarely than in the form of stripes and bundles of rays, the aurora sometimes appears in the form of undefined luminous clouds. The *black* rays of the northern lights, first noticed by Professor Hanstein, *i. e.* sharply bounded stripes, surrounded by masses of light, which, however, are quite detached from them, are a not unfrequent, but extremely striking phenomenon. Changes of temperature in the atmosphere generally stand in connection with the appearance of brilliant northern lights, and thus a dry cold occurs after northern lights emanating from the eastern horizon, while storms and snow with diminished cold, follow the western aurora. Frequently, however, the northern lights from the east and west appear simultaneously, without the one or the other gaining the mastery, and then unsettled weather ensues. A variation of the magnetic needle generally follows the appearance of the aurora; in the case of the eastern aurora the North Pole of the needle is turned eastwards, and in that of the western westwards. A difference of *height* has been distinctly observed, although measurements were not practicable. In Mr Ihle's Journal, days are indicated on which the rays of the aurora were visible below the clouds, nay even *lower than the abruptly rising acclivity of the valley.* The lower the aurora and the nearer the zenith, so much the more powerful was its influence on the magnetic needle in relation to declination and intensity. It has also resulted from Ihle's observations, that there is a certain connection between the occurrence of storms and the appearance of the northern lights. In the case of violent storms occurring in sudden gusts, the Northern Lights are almost always in a state of flickering rapid movement; and, while at the periods of the greatest evolution of light, the storm is generally much diminished, it is renewed in all its strength when the the brilliancy of the aurora

2. *Mr Hopkins on the Cause of the Motion of Glaciers.*—Mr Hopkins, at the Cork meeting of the British Association, explained his views respecting the cause of the motion of glaciers. De Saussure had adopted the theory which attributes this motion to the resolved part of gravity, acting along the inclined surfaces on which all glaciers in motion are situate; and he explained, also, how the motion could be facilitated by the effects of the internal heat of the earth, and of sub-glacial currents. When the attention of philosophers, however, was recalled a few years ago to this subject, and more accurate observations and admeasurements were made, the inclinations of the beds of glaciers were found, in many cases, to be so small (in the glacier of the Aar, for example, not exceeding three degrees) that it appeared extremely difficult to conceive how the force of gravity alone could be adequate to overcome the friction on the bottom and sides of the glacier, and the numerous local obstacles to its movement. Numerous experiments on the descent of bodies along inclined planes had shewn, that, when the surfaces of the bodies and planes were perfectly hard and polished, no motion would ensue without an inclination considerably greater than that of many glaciers; and, moreover, that the inclination required to produce motion was independent of the weight of the sliding body. These considerations led to the very general rejection of De Saussure's theory, and to the adoption, by many persons, of the *dilatation theory*, of which M. Agassiz had been the principal advocate. According to this theory, a part of the water produced by the dissolution of the superficial portion of the glacier during summer, passed, by infiltration, into the minute pores and crevices of the glacier, where it was again converted into ice, and, by its expansion in the process of freezing, produced a dilatation and consequent motion of the glacier. It was manifest, however, that the frequent alternation of freezing and thawing within the glacier which this theory assumed, could not possibly take place at depths beneath its surface exceeding a very few feet, and therefore could not produce any sensible effect on the motion of the whole mass. This theory presented many other difficulties, of which no adequate solution had been given, and the author could not but consider it as contrary to the most obvious mechanical and physical principles.

Another theory had also been put forward, which attributed the motion of glaciers to the expansion of water in the act of freezing after it had filled, not the minute pores of the ice, but internal cavities of considerable dimensions.* But, since the temperature of the glacier, at considerable depths, must be sensibly constant, how were new cavities to be formed when existing ones had been once filled up? The author, regarding both this theory and the preceding one as untenable, was thus led to examine how far the apparent objections to De Saussure's theory were really valid, by a series of experiments on the descent down inclined planes. The experiments were made in the following manner:—A slab of sandstone, prepared to be laid down as a part of a common flagstone pavement, was so arranged as to be easily placed at any proposed inclination to the horizon. The surface of the slab, so far from being polished, retained the grooved marks of the instrument with which the quarryman had shaped it. A quantity of ice was placed on the slab, within a frame nearly a foot square, intended merely to keep the ice together, and not touching the slab, with which the ice alone was in contact. The following were results obtained in one set of experiments, the ice being loaded with a weight of about 150 lb.:—

Inclination of the planes,	3°,	6°,	9°,	12°,	15°.
Mean space for one hour,	0.31,	0.62,	0.96,	2.0,	2.5 inches.

When the weight was increased, the rate of motion was also increased. The least inclination at which sensible motion would take place, was not determined; but it was ascertained that it could not exceed *half a degree* in the case of a smooth but unpolished surface. With a polished surface of a marble slab, the motion of the ice indicated a deviation from horizontality, with as much sensibility as water itself. It will be observed, in the results above given, that (1.) the motion was unaccelerated, and (2.) it increased with the inclination, and (when the inclination was not greater than nine or ten degrees) in nearly the same ratio; and (3.) the rate of movement was of the same order of magnitude as in actual glacial motion, which may be stated generally, in cases yet observed, never to exceed two feet a-day. The extreme small friction between the plane and the ice, indicated by the small inclination necessary to produce motion, was manifestly due to the circumstance of the lower surface of the ice being in a state of gradual disintegration, which, however, was extremely slow, as proved by the small quantity of water proceeding from it. In the application, therefore, of these results to the case of actual glaciers, it was necessary to shew that the temperature of their lower surfaces could not generally be less than

32° Fahr. Such, the author stated, must necessarily be the case, unless the conductive power of ice was greater than it was deemed possible that it could be. He considered the sub-glacial currents as powerful agents in the disintegration of the lower surfaces of glaciers, especially near their lower extremities. The results of Professor Forbes's observations on the motion of the Mer de Glace of Mont Blanc, afforded, as regards that glacier (and, by inference as regards all other glaciers) a complete refutation of the theories which attribute glacial movements to any expansion or dilatation of the ice. The Professor had, however, put forth a new theory, which agreed with that offered by Mr Hopkins in attributing glacial motion to the action of gravity, but differed from it entirely as a mechanical theory in other respects. The Professor appeared to reject the sliding theory of De Saussure, on account of the difficulties already mentioned (which were now removed by the above experiments), and assigned to the mass of a glacier the property of *plasticity, or semifluidity,* in a degree sufficient to account for the fact of its descending down surfaces of such small inclination. According to this theory, the motion was due to the small cohesion of one particle of glacial ice to another. Mr Hopkins stated his conviction that the internal cohesion of the mass was immensely greater than its cohesion to the surface on which it rests, whenever the lower surface is in a state of disintegration. It was perfectly consistent with this conclusion, to assign to the glacier whatever degree of *plasticity* might be necessary to account for the relative motions of its central and longitudinal portions under the enormous pressure to which, according to his theory, he shewed it might be subjected. Such relative motions, however, were probably facilitated more by the *dislocation* than the plasticity of the mass. Sufficient, he trusted, had been advanced to prove that the *sliding theory* assigned a cause adequate to the production of all the observed phenomena of glacial movements.—*Athenæum.*

3. *On the Transport of Erratic Blocks and Detritus from the Alps to the Jura.* By Mr Hopkins.—With respect to the transport of erratic blocks and detritus from the Alps to the Jura, Mr Hopkins observed that the greatest height which glaciers had formerly attained in the valley of the Rhone (whence a large portion of the erratics had been derived), appeared to be well defined by lateral moraines and polished rocks, while the greatest height at which these blocks had been deposited on the Jura was also well defined. Thus, according to M. Charpentier, the Rhone glacier must have risen, at the mouth of the

Valais, to about 2500 feet above the existing surface of the Lake of Geneva; while the highest band of detritus on the Jura was stated to rise to a still higher level. It was inconceivable, therefore, that such detritus should have been lodged at its present elevation by former glaciers. The only way in which it appeared possible to obviate the mechanical difficulties of the subject, was to suppose the transport to have been effected when the Jura was at a lower level relatively to the Alps, and the whole district lower relatively to the surface of the ocean. In such case, the space between the Alps and the Jura may have been occupied by the sea, and the ice, with its transported materials, may have passed from the former to the latter chain, partly with the character of a glacier, and partly with that of an iceberg. This hypothesis is perfectly consistent with the supposition of the general configuration of the surface of the Jura having been the same at the epoch of the transport as at the present time; and Mr Hopkins believed it would be found equally so with all the observed phenomena of that region.—*Athenæum*, No. 827, p. 803.

4. *On the Agency of Glaciers in Transporting Rocks.*—Colonel Sabine, at the Cork Meeting, related, in illustration of the agency of glaciers in transporting rocks, that when the Antarctic Expedition had reached 79 degrees S. latitude, the vessels were stopped by a barrier of ice, from 100 to 180 feet in height, and 300 miles from east to west; beyond these cliffs they discovered a range of lofty mountains, 60 miles from the sea, the westernmost of which appeared to be 12,000 feet in height. From the face of these ice-cliffs vast masses were constantly breaking off, and floating northward, bearing with them fragments of the rocks which had been derived from the mountains. In the latitude of 66 and 67 degrees, a distance of 700 miles from the glaciers, these floating icebergs appeared to be usually arrested, so as to form a floating barrier, at which ships were often stopped; and it had been observed, that, between this zone and the cliffs, the sea deepened considerably. Over all this area the icebergs would be constantly strewing masses of rock and detritus, particularly at their northern limit, where they would probably form mounds resembling terminal glacial moraines. Colonel Sabine then described similar phenomena in Baffin's Bay, which he stated to be, in most parts, deeper than the thousand-fathom line, but shallow at the strait which forms its entrance. The bay was surrounded by alternate cliffs of rock and valleys occupied by glaciers, and presented cliffs of ice along the shore, from which masses became detached and floated off to the zone of shallow water at the entrance of the bay, where

they were constantly becoming arrested in their progress, and deposited the fragments of granite, trap, and limestone containing fossils, derived from the shore.—*Athenæum*, No. 827, p. 803.

GEOLOGY.

5. *Mr Murchison on the Permian System, as applied to Germany, with Collateral Observations on Synchronous Deposits in other Countries; shewing that the Rothe-todte-liegende, Kupfer-Schiefer, Zechstein, and the lower portion of the Bunter-sandstein, form one natural group, constituting the upper member of the Palæozoic Rocks.*—The word *Permian*, as remarked at page 115, when first proposed by Mr Murchison, was intended to distinguish a natural group of deposits, lying between the well-known carboniferous strata beneath, and the less perfectly defined trias above it. He first suggested that the group (so designated from the ancient kingdom of Permia, which is exclusively occupied by it) should combine those deposits known under the name of Rothe-todte-liegende (lower new-red of England), Kupfer-Schiefer, Zechstein, &c. (magnesian limestone, &c.) Subsequently, however, he was disposed to doubt whether it might not be more correct to class the Rothe-todte-liegende with the coal-bearing deposits beneath it, than with the Zechstein, because the plants of the lower red sandstone could not be distinguished from those of the coal-measures. Revisiting Hesse, Saxony, Silesia, the Thuringerwald, and other parts of Germany, Mr Murchison has obtained what he considers proof that the Rothe-todte-liegende is part and parcel of the same natural group as the Zechstein, and must, therefore, be considered part of the Permian system. He has also convinced himself that the great deposit hitherto known under the denomination of Bunter-sandstein, Grès bigarré, or new-red sandstone, should be divided into two parts, the lower of which ought to be classed with the Permian, and separated from the trias, with which it had been merged. To prove the first of these positions, or that the Rothe-todte-ligende is a part of the Permian group, Mr Murchison cited the order of succession in numerous sections in Germany, shewing an uninterrupted sequence from the lower red conglomerate sandstone and shale, into the overlying Zechstein. It is a question whether the plants of these lower red rocks can be distinguished as a whole from those of the subjacent coal-measures, plants being, as yet, the only organic remains found in them. From his observations in Saxony, and particularly from an inspection of the fossil

plants collected and partly described by Captain Gutbier, Mr Murchison believes that such a separation exists. Among the coal-plants of Saxony are forms of Neuropteris, closely approaching to, if not identical with, those species which occur in the Permian rocks, whilst there is no trace of the common plants of the underlying coal. These plants being imbedded in a whitish or cream-coloured finely levigated clay-stone, and their leaves being brought into beautiful relief by being invariably as green as if they had been peculiarly and happily dried in an herbarium, form admirable subjects for the most precise distinctions of the fossil botanist. In Silesia (at Ruppendorf, and other localities west of Waldenberg, between Breslau and Glatz) there is a fine development of strata from the base of the Rothe-todte-liegende (where that deposit overlies a productive coal-field based upon true mountain limestone) into other red sandstones and shales, which have a marked aspect, from being interlaced with bands of black, bituminous, thin, flaggy, limestone. Though doubts had been entertained as to the age of this limestone, Mr Murchison does not hesitate to consider it as the equivalent of the Zechstein, and the whole red group, of which it forms a member, as the counterpart of the Permian system; for, besides its very clear position, this calcareous flag-stone contains plants and fishes similar to those of the Permian rocks of Russia. Among the former, the *Neuropteris conferta, nov. spec.*, of Göppert, has been identified with the most common fern brought from Russia. The most abundant fish, is *Palæoniscus Wratislaviensis*, Ag. On this occasion Mr Murchison passed rapidly over the zoological proofs that the Zechstein and Kupfer-Schiefer of Germany are the equivalents of the calcareous beds of the Permian system of Russia, as these had been given in detail in memoirs read before the Geological Society. He stated, however, that his opinion was now perfectly in harmony with that of Professor Phillips, namely, that the fauna of Zechstein, or magnesian limestone, has so much of the same general zoological type as the carboniferous limestone, that it must also form part of the Palæozoic series. Mr Murchison then proceeded to consider the age of these lower beds of the Bunter-sandstein, which had been hitherto included in the trias, on lithological evidence only. They contain no fossils either in Hesse, Saxony, or the Thuringerwald, where the Zechstein and Kupfer-Schiefer are most developed; and from all Mr Murchison's inquiries and observations, it appears that the upper mass only of the Bunter-sandstein contains the remains of plants and animals analogous to those of the Muschelkalk, which rests upon it. The footmarks of the Cheriotherium appeared also to be confined to the beds of sandstone, at a very little depth below the Muschelkalk. From these circumstances Mr Murchison was induced to regard the upper beds alone of the Bunter-sandstein as belonging to the trias; whilst the lower portion, which, though generally unfossiliferous, contained, in Russia, the same groups of fossils with the Permian rocks, he proposed henceforth to separate from the secondary system, and consider it, together with the Zechstein and Rothetodte-liegende, as the upper member of the Palæozoic series, supposed to be represented by a thin band of dolomite. The plants of the Permian system of Russia appear, from the opinion of M. Adolphe Brongniart, to possess a peculiar character; but they are still closely allied to carboniferous forms, like the plants of the Rothetodte-liegende of Saxony; and this evidence is in complete harmony with that afforded by the molluscs, corals, and ichthyolites. In conclusion, Mr Murchison remarked that the English strata, ranging under the synonym of Permian, formed a well-defined tract, separating the coal-fields from the newer deposits of red sandstone and marl; and as the magnesian limestone does not often appear in the form of a continuous deposit, it was the more desirable to give a certain latitude to this group, and not to define it too narrowly by mere mineral characters.

6. *On the Rise of the Coast of Scandinavia.*—Major L. Beamish, F.R.S., read a paper " On the apparent fall or diminution of Water in the Baltic, and elevation of the Scandinavian Coast." During a journey to Stockholm, in the early part of the present summer, the author had occasion to see and hear much respecting the diminution of water in the Baltic, a practical and personal evidence of which he experienced in the harbour of Travemunde, on the 4th of May, by the sudden fall of water at the port, which took place very rapidly, and to a great extent. The steamer, which ought to have left Travemunde on the 18th, was detained by this cause until the 21st. It is well known, that, although without tide, the Baltic is subject to periodical variations of depth, but the water has fallen, during the present summer, to a degree far below these ordinary variations; and the fact was considered so remarkable as to be thought worthy of being brought before the notice of the Swedish Academy of Sciences, by Baron Berzelius, in July last. This fall or diminution of water was already perceptible in the summer of 1842, since which, the Baltic has never returned to its average mean height: but, on the contrary, has diminished, and there seems now no probability that the former level, or the height of 1841, will be again attained. Meantime,

no perceptible change has taken place in the waters of the North Sea, and the unscientific observer asks, What has become of the waters of the Baltic? The answer is probably to be found in a simultaneous phenomenon apparent on the Swedish coast, the gradual elevation of which has been satisfactorily proved by the personal observation of Mr Lyell. Recent observations, however, would seem to shew that this elevation does not proceed at any regular or fixed rate, but, if he might use the expression, *fitfully* at uncertain periods, and at a rate far greater than was at first supposed. At the same meeting, when Baron Berzelius drew the attention of the Swedish Academy to the diminution of water in the Baltic, a communication was made from an officer who had been employed on the south-west coast of Sweden, in the Skärgard of Bohuslän, north of Gottenburgh, giving evidence of the recent elevation of that part of the coast, and stating, that during the present summer, fishermen had pointed out to him near the Malström, at Oroust, *shoals* which had never before been visible. The elevation of the Swedish coast forms a striking contrast with the unchanged position of the contiguous coast of Norway, which, as far as observation has hitherto been extended, has suffered no change within the period of history, although marine deposits, found upon the Norwegian Hills, at very considerable elevations above the level of the sea, prove that those parts were formerly submerged. More accurate information, however, will, before long, be obtained on this interesting point, as a commission has been appointed by the Norwegian government to investigate the subject, and marks have been set up on the coast which will, in a few years, afford the desired information; meantime, the Scandinavian peninsula presents an extraordinary phenomenon; the western, or Norwegian side, remaining stationary, while the south and east, or Swedish sides, are rising, and that, as the author had endeavoured to shew, at no inconsiderable rate. *Athenæum, No.* 829, p. 850.

ZOOLOGY.

Note on a vermineous alteration in the Blood of a Dog, caused by a great number of Hematozoary Animals of the Genus Filaria. By MM. Gruby and Delafond.—Physiologists and anatomists have long since proved the existence of certain entozoa in the nutritive fluid of cold-blooded animals, such as frogs and fishes. Among the Mammifera, also, worms have been sometimes found in the blood; but it is probable that they found their way thither merely by perforating the organs in which they were developed. It is of great importance for physiology, pathology, and natural history, to shew, not only the existence of entozoary worms in the blood, but to prove besides that they continually circulate in this fluid, in animals which, in their structure, make some approach to man. Now, as science is still destitute of an instance proving in an absolute manner the circulation of worms in the blood of Mammifera, we are anxious to communicate to the Academy the discovery we have made of entozoa circulating in the blood of a dog of vigorous constitution, and apparently in a state of good health.

These worms are from $0^{\text{mm}}.003$ to $0^{\text{mm}}.005$ in diameter, and about $0^{\text{mm}}.25$ in length. The body is transparent and colourless. The anterior extremity is obtuse, and the posterior or caudal extremity terminates in a very slender filament. In the anterior part a small furrow is observable, about $0^{\text{mm}}.005$ in length, which may be regarded as a buccal fissure.

In all its characters, this species of *hematozoaria* belongs to the genus Filaria.

The motions of these animals are very active. Life continues for no less than ten days after the blood has been extracted from the vessels, and placed in a vase at a temperature of 15 degrees of the centigrade scale. On examining a drop of blood under the glass of a microscope, we perceive these hematozaoria swimming about with an undulatory movement between the sanguineous globules, bending and unbending, twisting, and untwisting themselves with great vivacity.

In order to determine whether these worms existed in every part of the circulating stream, we examined the blood of the coccygian arteries, of the external jugulars, the capillaries of the conjunctive, the mucous vessels of the mouth, of the skin, and of the muscles, and in everywhere the fluid presented us with these entozoa.

For twenty days we daily opened the capillary vessels of different parts of the skin, and the mucous vessels of the mouth, and always ascertained the presence of these animals.

The urine and excrementitial matter contained none of them.

The diameter of the globules of the blood in a dog is from $0^{\text{mm}}.007$ to $0^{\text{mm}}.008$, that of the Filaria is from $0^{\text{mm}}.003$ to $0^{\text{mm}}.005$. There is not, therefore, the least doubt that this worm can circulate where-ever the blood requires to pass. After many researches undertaken for the purpose of ascertaining the quantity of blood existing in the vessels of dogs of medium size, we estimated that the dog in question had $1^{\text{kil}}.500$ of blood in circulation. Now a drop of this blood weighs $0^{\text{kil}}.067$, and in this drop there are usually from four to five filaria.

In the whole blood of this dog, there must, therefore, have been more than 100,000 of these worms.

The prodigious number of these animals is the more surprising from the dog appearing to enjoy good health. However, we have remarked that the entozoa of the digestive tube of dogs, the taenia, rarely derange the vital functions, even when in very great numbers.

During the last year, we have examined the blood of from 70 to 80 dogs without meeting with the Filaria, and, from the date of its discovery, we have sought for it in vain in the blood of 15 dogs.*

8. *Examination of Tartar and the Mucous Coatings of the Tongue and Teeth.*—These coatings, which, according to Laugier and Vauquelin, are chiefly formed of carbonate and phosphate of lime, agglutinated by a little cement, have been studied microscopically by M. Mandl, who, having macerated them, in a fresh state, in distilled water, discovered a large quantity of *vibrious baguettes.* This observer consequently believes, that such coatings are formed of calcareous skeletons of these infusoria. Leuwenhoek long ago indicated the presence of infusoria in the same products.—*Report of Proceedings of the Academy of Sciences in "L'Institut."*

9. *Light of the Glow-Worm.*—M. Matteucci lately addressed a notice to the Academy of Sciences, containing the results of experiments which he has made on the phenomena constituting the phosphorescence of the glow-worm. The following is the substance of his communication:—Carbonic acid and hydrogen are the media in which the phosphorescent matter ceases to shine after a space of 30 or 40 minutes, if the gases are pure. In oxygen gas the light is more brilliant than in atmospheric air, and it remains brilliant for nearly triple the length of time. When it shines in the air or in oxygen, it consumes a portion of oxygen, which is replaced by the corresponding volume of carbonic acid. In the same media, when there is an impossibility of light being emitted, there is no oxygen absorbed, and no carbonic acid developed. Heat augments to a certain extent the light of the phosphorescent matter, whereas cold produces the opposite effect. When the heat is too great, the substance is altered. The same thing takes place when it is left in the air or in some gases for a certain time; that is, when the substance is separated from the animal. This matter, so altered, is no longer capable of emitting light or of becoming luminous. According to these facts,

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* From *Ann. de Chimie et de Physique*, t. vii. p. 381.

it would seem that the phosphorescence of the glow-worm is a phenomenon of combustion, the result of the combination of the oxygen of the air with carbon, which is one of the elements of the phosphorescent matter.—*L'Institut*, No. 503.

10. *On the Structure of the Skin in the different races of Man, and new proof of the common origin of all the Varieties of the Human Species.*—The following is an abstract of the results of M. Flourens' researches on the comparative structure of the skin in the different human races, which he communicated along with illustrative drawings. One of the figures represents the skin of an individual of the white race, which is seen to be composed of three layers or distinct membranes, the dermis and the two epiderms; and, between the second or internal epidermis and the dermis, there is no trace visible of a pigmental layer—no trace of a pigmentum. Other figures represent the skin of a Kabyle, a Moor, and an Arab. All these skins are of a bistre colour, but in general this colour is deeper in the Arab than in the Moor, and in the Moor than in the Kabyle. Except in this respect, every thing in their structure is similar: in all there are two epiderms and a dermis, and in all there is a layer of pigmentum, and a pigmental membrane between the second epidermis and the dermis. There is also a representation of the skin of a cross of an Arab and a Negress; and another of the skin of a Negro. They exhibit the same structure as the skin of the Kabyle, the Arab, and the Moor. One drawing of the skin of an Arab, affected by partial albinism, is specified by M. Flourens as meriting particular attention. Upon that skin there are many white spots; and at the side of these spots the skin preserves its ordinary colour, which is blackish or bistre-coloured. Now, where the skin is black, there is a well-marked pigmentum, but where the white spots occur, there is no pigmentum. Judging, then, from this example, the malady termed albinism would only tend to produce the absence—the non-secretion of the matter which constitutes the pigmentum. With regard to all these preparations of the skin, M. Flourens makes the following general remarks:—If we compare the structure of the skin in all these races, viz. the Kabyle, the Arab, and the Moor, on the one hand, and, on the other, the American and the Negro, we find that it is every where the same. So that the Kabyle, the Arab, and the Moor, who probably belong to the Caucasian or white race, but certainly do not belong either to the red or to the black race, have, nevertheless, a pigmental apparatus perfectly similar to that of the black and the red races. The white

man himself has his pigmental apparatus, very circumscribed it is true, but still quite distinct, in the nipple, particularly in the female. Having submitted the coloured portion of the skin of the breast to his anatomical processes, M. Flourens found, first of all, two epiderms, and then, between the second epidermis and the dermis, a pigmental membrane and a layer of pigmentum; in a word, the whole pigmental apparatus. One of the figures represents this. Thus, he continues, we have a portion of the skin of the white man, in which we find the whole structure of the skin of the coloured races. Have we not in these facts a new proof, a direct proof, of the common origin of the various human races, and of their original unity? To this proof, derived from the study of the skin, M. Flourens intends adding another, deduced from the study of the skeleton, and especially of the cranium.—*L'Institut*, No. 504.

11. *Irish and Alpine Hares said to be varieties of the same species.*—At the Cork Meeting of the British Association, Mr Thomson exhibited specimens of the Alpine hare *(Lepus variabilis)*, from the Highlands of Scotland, and of the hare of Ireland *(Lepus Hibernicus)*, for the purpose of showing that the species are identical. Of this fact he, judging from external characters, satisfied himself last autumn, when in the Highlands of Scotland, and subsequently proved it by a comparison of the anatomical characters of the two supposed species.

NEW PUBLICATIONS RECEIVED.

1. Considérations Geologiques sur le Mont Salève, et sur les Terrains des Environs de Genève, par Alphonse Favre, Membre de la Société de Physique et d'Histoire Naturelle de Genève, &c. &c. 4to, with plates. Geneve, 1843.

2. Statistical Account of Scotland, No. 44, containing the County of Linlithgow complete, and part of the County of Perth. July 1843.

3. Account of the Museum of Economic Geology. By T. Sopwith, F.G.S., &c.

4. On the Bebeeru Tree of British Guiana. By Douglas Maclagan, M.D., F.R.S.E. 4to. 1843.

5. On the Medicinal Properties of Bebeeru Bark. By Douglas Maclagan, M.D. 8vo. 1843.

6. Bulletin de la Société Geologique de France. T. xiv., f. 21–24. (20 Mars—17 Avril 1843.)

7. Die ausgleichungs—Rechnungen der practischen Geometrie, &c. Von C. L. Gerling. Hamburg and Gotha. F. and A. Perthes. 1843.

8. Transactions of the American Philosophical Society, held at Philadelphia, for promoting Useful Knowledge. Vol. viii. Parts I. II. III. New Series, 4to, pp. 357. Philadelphia, 1841-2-3.

9. Proceedings of the American Philosophical Society for November and December 1842, and January, February, and March 1843. 8vo.

10. Description of the Skeleton of an extinct Gigantic Sloth, *Mylodon robustus*, Owen, with Observations on the Osteology, Natural Affinities, and probable Habits of the Megatherioid Quadrupeds in general, with numerous plates. By Richard Owen, F.R.S., &c. &c. John Van Voorst, 1 Paternoster Row, London.

11. Applications of the Electric Fluid to the Useful Arts. By Mr Alex. Bain; with a Vindication of his Claim to be *the First Inventor* of the Electro-Magnetic Printing Telegraph, and also of the Electro-Magnetic Clock. By John Finlaison, Esq., Actuary of the National Debt Office, and Government Calculator. 8vo, pp. 127, with plates. London, Chapman and Hall, 186 Strand. 1843.

12. Principles of Mathematical Geography, comprehending a Theoretical and Practical Explanation of the Construction of Maps, with rules for the Formation of the various kinds of *Map-Projections*, and an Appendix on Physical Geography. By W. Hughes, F.G.S., Professor of Geography in the College of Civil Engineers, &c. &c. London, John Weale. 12mo, pp. 157, with maps, &c. &c. *This useful volume, which we recommend to the attention of our readers, we owe to Mr Hughes, a young gentleman, who has already raised himself to eminence as a map-constructor and geographer, and who promises ere long to occupy a prominent position among the geographers of our country.*

13. The American Journal of Science and Arts, conducted by Professor Silliman, and Benjamin Silliman jun., up to July 1843.

14. Annalen der Physik und Chemie. Herausgegeben zu Berlin, Von J. C. Poggendorff. Received up to No. 5, 1843.

15. A Catalogue of British Fossils, comprising all the Genera and Species hitherto described, with references to their Geological Distribution and to the Localities in which they have been found. By John Morris, London. John Van Voorst, Paternoster Row. 1843. 8vo, pp. 216, with an ample Index. *This valuable volume ought to find a place in every Geological Library.*

16. The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland for July 1843. Blackwood and Sons, Edinburgh and London

17. Essai sur l'Influence des Comètes sur les Phenomenes de la Terre. Par Thomas Ignace Marie Forster, M.B., F.L.S. and Ast. S. of London, &c. 8vo, pp. 128. Bruges, 1843.

18. Discours Preliminaire sur l'etude de l'Histoire Naturelle. Par T. J. M. Forster, M.B., &c. Bruges, 1843.

19. Bibliothèque Universelle de Genève, up to No. 91, July 1843, inclusive.

GERMAN WORKS PUBLISHED.

1. G. Valentins Lehrbuch der Physiologie des Menschen. Gr. 8vo. 1843.

2. Sars M. Zoologica Norwegica. 1 liv., with 10 coloured folio plates. Christiania, Dahl. 1843.

3. Keilhau's Gæa Norwegica. 2 liv., folio. Christiania, Dahl. 1843.

4. J. C. A. Kraemer Observationes Microscopicæ et Experimenta de motu Spermatozoorum. Dissertatio inaug. Physiologica. Adjecta est Tabula Aenea. Gottingæ, 1843.

5. Kraus. L. A. Etymologish-Medicinisches Lexicon. Of this valuable work a third and much enlarged edition is at present in the press.

6. M. P. Erdl, Tafeln zur Vergleichenden Anatomie des Schädels. 20 Tafeln mit Erläuterndem Texte. Imp. fol. Munich, Palm. 1843.

7. Atlas der Cranioscopie, oder Abbildungen der Schädel und Antlitzformen berühmter oder sonst Merkwürd. Personen. Heft 1. enth. auf 10 lithog. tafeln die Abbild. der Kopfformen: Schillers, Talleyrands, eines Grönländers, eines Cretins, Napoleons, eines alten Skandinaviers, eines Kaffern, und eines Bali, so wie Zwei Tafeln übereinander Gezeichn. Contour dieser Köpfe. mit Deutschem und Franz. Text. Folio. Leipzig. Weichardt, 1843.

8. C. G. Carus; Goethe, seine Individualität und sein Verhältniss zu den Naturwissenschaften, dargest. unt: Beifügung Zwanzig eigenhand. (noch ungedr.) Briefe Goethes an den Verf. gr. 8. Leipzig, Weichhardt. 1843.

WORKS ABOUT TO APPEAR.

1. A Periodical under the title " The London Physiological Journal, or Monthly Record of Observations on Animal and Vegetable Anatomy and Physiology, chiefly made by the aid of the Microscope," by Mr E. J. Quekett, F.L.S., assisted by Dr Goodfellow, is about to be published by M. Van Voorst.

2. Crustacealogical Researches. Part I. By H. D. S. Goodsir, Keeper of the Museum of the Royal College of Surgeons in Edinburgh. This first part is the commencement of a series of memoirs for the purpose of illustrating the anatomical structure and natural history of the Crustacea.

List of Patents granted for Scotland from 23d June to 23d September 1843.

1. To William Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, being a communication from abroad, "certain improvements in the preparation of paper designed for bank notes, Government documents, bills, cheques, deeds, and other purposes, wherein protection and safety from forgeries or counterfeits are required."—23d June 1843.

2. To William Needham, of Birmingham, in the county of Warwick, gunsmith, "improvements in fire-arms."– 4th July 1843.

3. To Robert Smart, of Commercial Road, in the parish of Saint Mary Radcliff, in the city of Bristol, ship-owner, "improvements in paddle Wheels."—4th July 1843.

4. To Luke Hebert, of Dover, in the county of Kent, civil engineer, "certain improvements in mills or machines for the grinding and dressing, or reducing and separating, grain and other substances."—13th July 1843.

5. To James John Greer, of Woolwich, in the county of Kent, surgeon, "improvements in apparatus for securing or fixing standing rigging, and chains, and other tackle."—14th July 1843.

6. To Alonzo Grandison Hull, of Clifford Street, in the county of Middlesex, Doctor of Medicine, "improvements in electrical apparatus for medical purposes, and in the application thereof to the same purposes."—15th July 1843.

7. To Joseph Daniel Davidge, of Greville Street, Hatton Garden, in the parish of Saint Andrew Holborn, and county of Middlesex, machinist, "improvements in manufacturing certain materials as substitutes for whalebone, applicable to various useful purposes, and in the machinery for effecting the same."—17th July 1843.

8. To George Parsons, of West Lambrook, in the county of Somerset, gentleman, and Richard Clyburn of Uley, in the county of Gloucester, engineer, "certain improvements in machinery for beating, cleansing, and crushing various animal and vegetable materials or substances."—18th July 1843.

9. To Sir John Scott Lillie, of Chelsea, in the county of Middlesex, Knight and Companion of the Most Honourable Order of the Bath, certain improvements in roads."—19th July 1843.

10. To Richard Laming of Radley's Hotel, New Bridge Street, Blackfriars, in the city of London, gentleman, "certain improvements in the purification and application of ammonia to obtain certain chemical products."—19th July 1843.

11. To James Lancaster Lucena, of No. 4 Garden Court, Middle

Temple, London, barrister-at-law, of an extension of five years from 3d August next, of a patent granted to ELIJAH GALLOWAY, of King Street, in the borough of Southwark, engineer, "for improvements in steam-engines, and in machinery for propelling vessels, which improvements are applicable to other purposes."—20th July 1843.

12. To JOHN GEORGE BODMER, of Manchester, in the county of Lancaster, engineer, "certain improvements in locomotive engines and carriages, to be used upon railways, in marine-engines and vessels, and in the apparatus for propelling the same, and also in stationary engines, and in apparatus to be connected therewith, for pumping water, raising bodies, and for blowing or exhausting air."—21st July 1843.

13. To THOMAS OLDHAM, of Manchester, in the county of Lancaster, manufacturer, "a certain improved mode of manufacturing bonnets and hats."—24th July 1843.

14. To GEORGE PARSONS, of West Lambrook, near South Petherton, in the county of Somerset, gentleman, "a portable roof for various agricultural and for other purposes."—26th July 1843.

15. To SAMUEL ELLIS, of Salford, Manchester, in the county Palatine of Lancaster, engineer, "certain improvements in weighing machines, and in turn-tables to be used on or in connection with railways, and in weighing machines to be used in other situations."—3d August 1843.

16. To CHARLTON JAMES WOLLASTON, of Welling, in the county of Kent, gentleman, being a communication from abroad, "improvements in machinery for cutting marble and stone."—3d August 1843.

17. To ERNST LENTZ, of Eastcheap, in the city of London, gentleman, being a communication from abroad, "improvements in machinery for raising and forcing water and other fluids, which machinery, when worked by steam or water, may be employed for driving machinery."—3d August 1843.

18. To EDMUND MOREWOOD, of Thornbridge, in the county of Derby, merchant, and GEORGE ROGERS of Chelsea, in the county of Middlesex, gentleman, "improved processes for coating metals."—8th August 1843.

19. To FRANCIS ROUBILLIAC CONDER, of Highgate, in the county of Middlesex, civil engineer, being a communication from abroad, "improvements in the cutting and shaping of wood, and in the machinery for that purpose."—9th August 1843.

20. To THOMAS EARL of DUNDONALD, of Regent's Park, in the county of Middlesex, "improvements in the rotatory or revolving engines, and in apparatus connected with steam-engines, and in propelling vessels."—10th August 1843.

21. To SAMUEL ECCLES, of Hulme, in the county of Lancaster, machinist, and MATTHEW CURTIS, of Chorlton-upon-Medlock, in the said county, machinist, "certain improvements in looms for weaving."—10th August 1843.

22. To Fennell Allman, of 9 Salisbury Street, in the county of Middlesex, surveyor, "certain improvements in apparatus for the production and diffusion of light."—14th August 1843.

23. To William Bates, of Leicester, fuller and dresser, "improvements in the dressing and getting up of hosiery goods and other looped fabrics made from merino, lamb's wool, worsted, cotton, silk, and other yarns."—16th August 1843.

24. To John Laird, of Birkenhead, in the county of Chester, shipbuilder, "improvements in the construction of steam and other vessels."—16th August 1843.

25. To Gregory Seale Walters, of Coleman Street, in the City of London, merchant, being a communication from abroad, "improvements in the manufacture of chlorine and chlorides, and in obtaining the oxides and peroxides of manganese in the residuary liquids of such manufacture."—16th August 1843.

26. To John Barnes, of Church, in the county of Lancaster, manufacturing chemist, and John Mercer, of Oakenshaw, in the county of Lancaster, calico-printer, "certain improvements in the manufacture of articles used in printing and dying cotton, silk, woollen, and other fabrics."—19th August 1843.

27. To John Burns Smith, late of Salford, in the county Palatine of Lancaster, but now of Stockport, in the county of Chester, cotton-spinner, "certain improvements in machinery, for preparing, carding, roving, and spinning cotton and other fibrous substances."—21st August 1843.

28. To James Overend, of Liverpool, in the county of Lancaster, gentleman, being a communication from abroad, "improvements in printing fabrics with metallic matters, and in finishing silk and other fabrics."—22d August 1843.

29. To John Wood, of Parkfield, Birkenhead, in the county of Chester, merchant, "certain improvements in machinery or apparatus for affording additional or artificial buoyancy to sea-going and other vessels, or for lessening their draught of water, and which said improvements are also applicable to raising vessels or other heavy bodies, and for securing or supporting the same."—23d August 1843.

30. To William Wylam, of the borough and county of Newcastle-upon-Tyne, merchant, being a communication from abroad, "an artificial composition, which, variously modified, may be applied in preparing fuel from coal and other substances, or as a cement, or as a substitute for stone, or as a coating for metals and other substances."—28th August 1843.

31. To James Greenshields, of Monteith Row, Glasgow, gentleman, "improvements in the manufacture of compositions for covering roads, streets, and other ways and surfaces, and in rendering fabrics waterproof, to be used for covering buildings, bales, packages, and for other useful purposes."—31st August 1843.

32. To Claude Edward Deutsche, of Fricourt's Hotel, St Martin's Lane, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in combining materials to be used for cementing purposes, and for preventing the passage of fluids, and also for forming or constructing articles from such composition of materials." —31st August 1843.

33. To William Wilson, John Steedholme Brownrigg, John Cockerel, and Sir George Gerard de Hochapich Larpent, Baronet, all of Belmont, in the Wandsworth Road, in the parish of Lambeth, in the county of Surrey, patent cocoa-nut, candle, and oil manufacturers, and cocoa-nut oil merchants, the assignees of James Soames junior, of Wheeler Street, Spittalfields, in the county of Middlesex, soap-maker, of an extension of three years, from the 17th December next, of a patent granted to the said James Soames junior, for "a new preparation or manufacture of a certain material produced from a vegetable substance, and the application thereof, to the purposes of affording light and other uses."—31st August 1843.

34. To Alexander Spears, of Glasgow, merchant, being a communication from abroad, "improvements on, or appertaining to glass-bottles, proper for wines and other liquids."—7th September 1843.

35. To Fredrick Steiner, of Hyndburn Cottage, near Accrington, in the county of Lancaster, turkey-red dyer, being a communication from abroad, "a new manufacture of certain colouring matter, commonly called 'Garancine.'"—7th September 1843.

36. To William Kenworthy, of Blackburn, in the county of Lancaster, cotton-spinner, "certain improvements in machinery or apparatus, called beaming or warping machines."—13th September 1843.

37. To Lemuel Wellman Wright, of Wrexham, in the county of Denbigh, North Wales, engineer, "certain improvements in machinery or apparatus for bleaching various fibrous substances, and is in possession of improvements in machinery or apparatus for converting or manufacturing the same into paper," being partly of his own invention, and partly communicated by a foreigner residing abroad.—20th September 1843.

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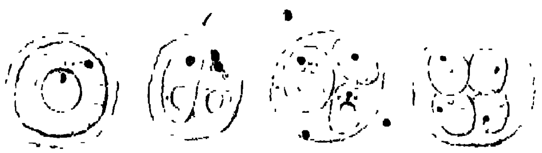
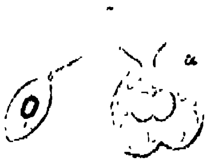


4

Chlamydomonas Pulvisculus



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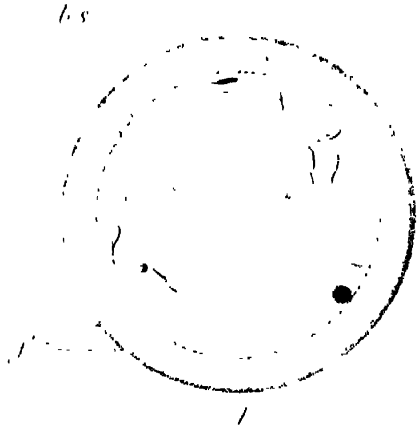
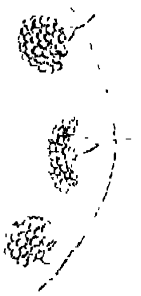
Monas vivipara.



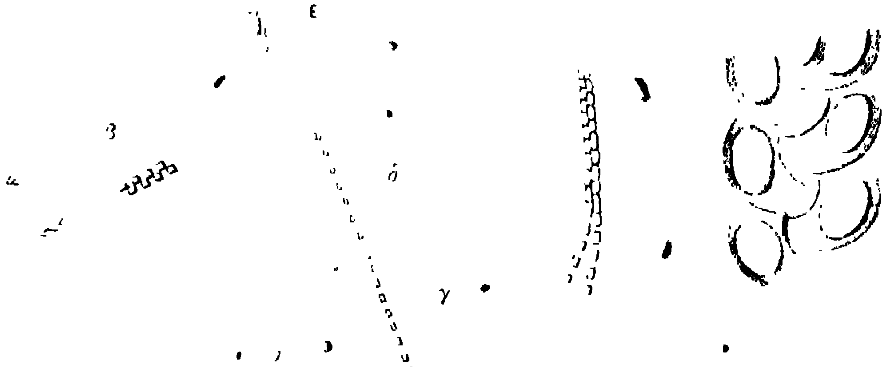
1

Ovum with Spermatozoa in its interior



6

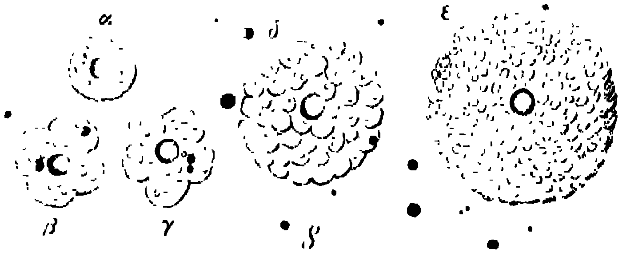
Pandorina Morum



Reproduction of the Muscular Fibre

7

Monas bicolor



8

Volvox Globator

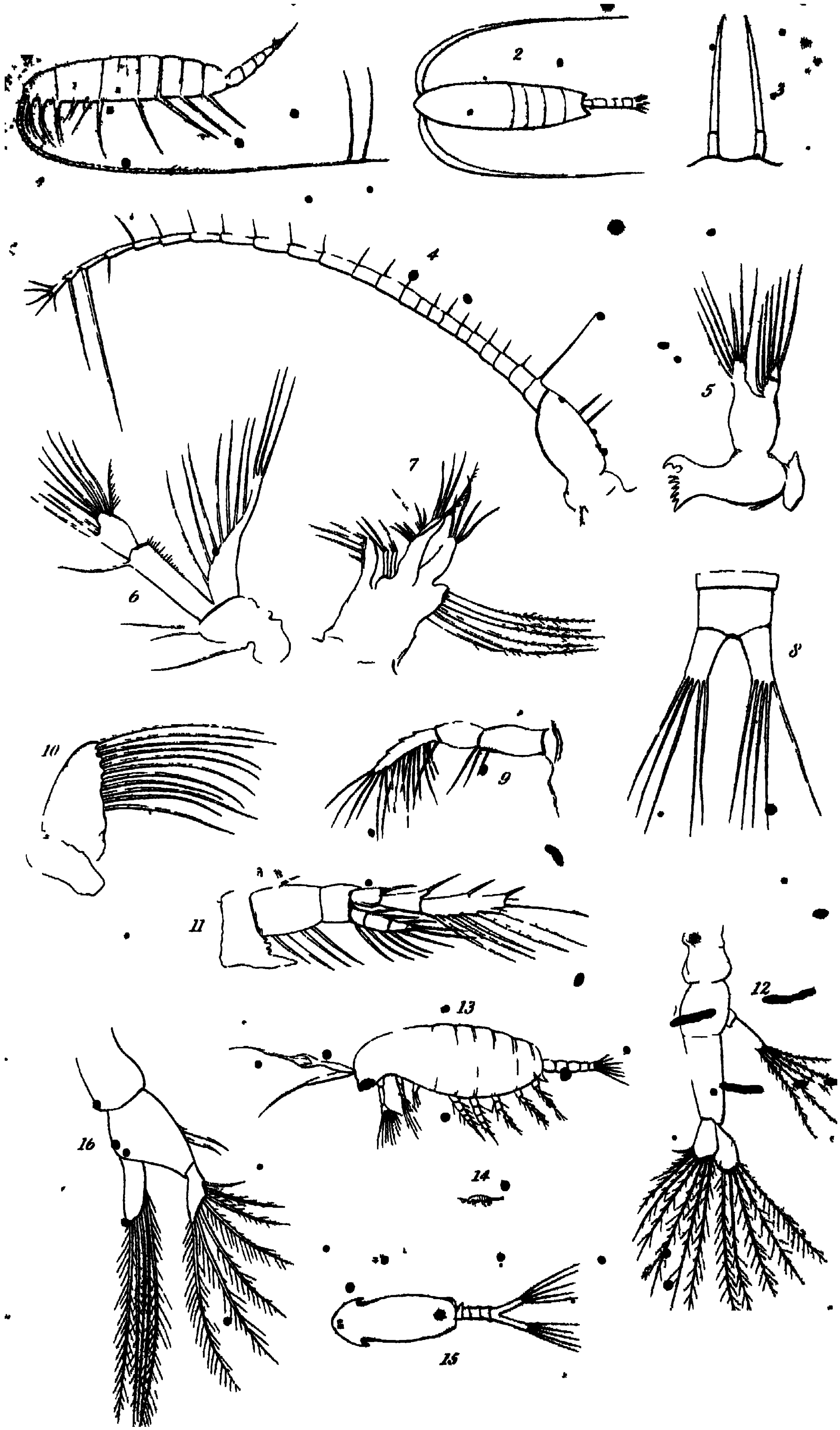


Plate IX

MAXTON'S LONG-SLIDE VALVE

FOR CONDENSING ENGINES

Fig 1

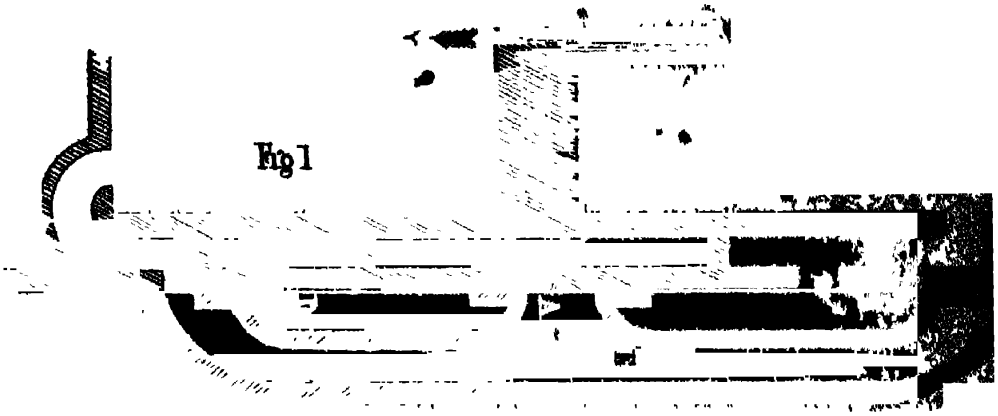


Fig 2

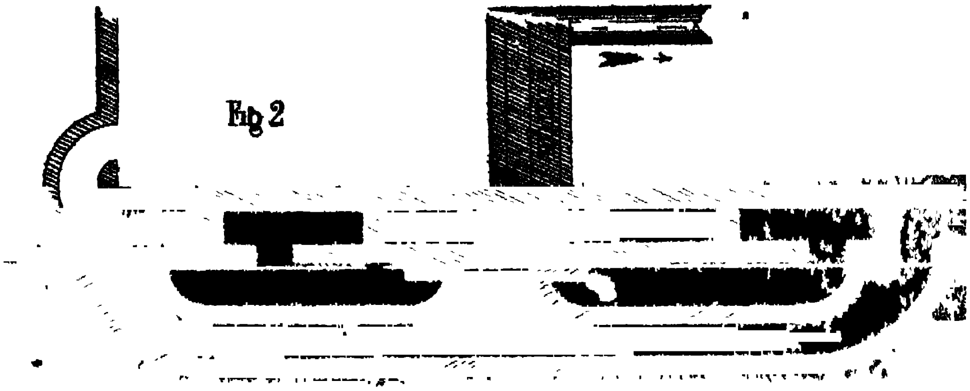


Fig 3